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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

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CONTENTS.

ART. I. Researches on the Electrical Currents in Metalliferous Veins, made in the mine of "Himmelfahrt," near Freyberg. By Professor F. REICH of Freyberg. Communicated by the Author, . 1

II. On the Glaciers of the Alps. By M. J. ANDRE' DE LUC, ... 15

III. On the Laurus Cassia of Linnæus, and the Plants producing the Cassia Bark of Commerce. By ROBERT WIGHT, M. D. . . . 20

IV. Observations on Cinnamon. By HENRY MARSHALL, F.R.S.E., Deputy Inspector-General of Hospitals, 72

V. On the Transverse Valleys or Openings in Chains of Mountains, which afford a Passage to Rivers. By M. J. ANDRE' DE LUC, . . 32

VI. On Rivers which break through Mountain-Chains. By the late Professor HOFFMAN of Berlin, 42

VII. Abstract of a Paper read to the Society of Arts, by Sir JOHN ROBISON, entitled, "A Narrative of Experiments and Suggestions, by the late Mr James Taylor, for the Application of Steam to Navigation;" with Introductory Remarks by Sir

MAY 26 '41 ALLEN

960997

John Graham Dalyell, President of the Society. Communicated by the Society of Arts for Scotland, ... 45

VIII. On the Habits of Testaceous Mollusca, belonging to the Genera Achatina and Phasianella. By F. Logan, Esq., Surgeon, R. N. Communicated by the Author, ... 47

IX. Zoological Instructions of the Royal Academy of Sciences, Paris, for the Scientific Expedition repairing to the North of Europe. Drawn up by M. Isidore Geoffroy Saint Hilaire. 53

Additional Notes to the Zoological part of the Instructions given by the Academy.

Note A On the Varieties of Locality, . 65

B On the Beaver, . . . 68

C On the Changes of Fur or Plumage according to the Seasons, . . 70

D On the Suspension of Life in the Batracians from the effects of cold, . . 71

X. Instructions in Zoology and Animal Physiology, for the British Antarctic Expedition, . . 72

XI. Meteorological Observations made at the Imperial Academy of Sciences of St Petersburg, from 1822 to 1835, and calculated by M. T. Kupffer, . 77

XII. On the Boracic Acid Lagoons of Tuscany. By John Bowring, LL.D., . . . 85

XIII. On the Supposed Existence of Fluoric Acid, as an Ingredient in certain Animal Matters. By G. O. Rees, M. D., F. G. S., &c. . . 90

XIV. Sketch of Animal Life in Nova Zembla (*Novaïa Zemlia*). By M. K. E. Von Baer, . 93

XV. Experiments on the Temperature of Vegetables with the Thermo-Electric Apparatus. By M. Dutrochet, ... 103

XVI. Register of Mean Temperature, &c. at Applegarth Manse, Dumfriesshire, Long. 3° 12′ W.; Lat. 55°

13′ N.; height above the sea 180 feet; distance from the sea 10 miles. Times of Observation, 9 A.M. and 9 P.M. By the Rev. WILLIAM DUNBAR. Communicated by the Author, . 112

XVII. Researches on the Structure of the Scales of Fishes. By Dr L. MANDL, . . . 113

Historical, ... 113

Scales considered as affording Characters for Classification, 122

XVIII. Report on the Manufacture of Tea, and on the Extent and Produce of the Tea-Plantations in Assam. By C. A. BRUCE, Esq. Superintendent of Tea-Culture. With a Map of the Tea Districts, 126

XIX. On the Calcareous and Siliceous Microscopic Animals which form the Chief Component Parts of Cretaceous Rocks. By Professor C. G. EHRENBERG, ... 161

XX. On the Extinction of Human Races. By Dr PRITCHARD, ... 166

XXI. On the Geographical Distribution of Insects. (Continued from vol. 27, p. 351), . . 170

XXII. On the Inflammable Gases of the Prussian Coal Mines. By Professor GUSTAV BISCHOF, . 183

XXIII. Description of a Polyphotal Lamp, and Reflector of Single Curvature, employed in Steam-vessels, Canal-boats, &c. By JOHN SCOTT RUSSELL, A. M., F. R. S. E., V. P. S. A., Communicated by the Society of Arts, ... 193

XXIV. Presentation of the Royal Society Medals to Dr M. BARRY and Mr IVORY, and the Copley Medal to Mr BROWN. By the President Lord Northampton, ... 196

XXV. Fossil Organic Remains in Transition Rocks in the Great Southern High-Land of Scotland, . 198

XXVI. Proceedings of the Wernerian Natural History Society. (Continued from vol. xxvi. p. 429), 199

XXVII. New Publications, ... 200

1. Journal of the Asiatic Society of Bengal. Edited by the acting Secretaries, ... 200
2. Madras Journal of Literature and Science. Published under the auspices of the Madras Literary Society, and Auxiliary Royal Asiatic Society. Edited by the Secretaries to the Asiatic Department, . . 201
3. Narrative of the Surveying Voyages of H. M. Ships Adventure and Beagle, between the years 1826 and 1836, describing their Examination of the Southern Shores of South America, and the Beagle's Circumnavigation of the Globe. Vol. iii. Also entitled, "Journal and Remarks, 1832—1836." By CHARLES DARWIN, Esq., M. A., Sec. Geol. Soc., London. H. Colburn. 8vo, pp. 615, . . . 201
4. Mineralogisch-geognostische Beschreibung der Umgegend von Schriesheim. Von GUSTAV LEONHARD. Stuttgard, 1839, ... 201

XXVIII. List of Patents granted for Scotland, from 17th September to 14th December 1839, . . 202

ERRATUM.

In Mr Nicol's paper on the Single Vision Prism of Calcareous Spar, vol. xxvii. p. 332, 6th line from bottom

For three-fourths *read* three-tenths

TO CORRESPONDENTS.

The Communication on a New Sympiesometer, and those on other subjects, are under consideration.

CONTENTS.

ART. I. On Daguerreotype. By ANDREW FYFE, M. D., F. R. S. E., F. S. A., &c. Communicated by the Society of Arts of Scotland, . Page 205

II. On the Nile, and the present and former Levels of Egypt. By Sir J. GARDNER WILKINSON. With a Plate, ... 211

III. On the Geographical Distribution of Insects. (Concluded from page 183), ... 224

IV. Of the Diseases of France in their relation to the Seasons, or Sketch of a Medical and Meteorological History of France. By Dr FUSTER, . 238

V. On the Adulteration of Fixed Oils. By WILLIAM DAVIDSON, M. D., Glasgow. Communicated by the Society of Arts of Scotland, . . . 250

VI. On the Decolorization of Palm Oil. By WILLIAM DAVIDSON, M. D., Glasgow. Communicated by the Society of Arts of Scotland, . . . 256

VII. On the Removal of the Fetid Odours of Fish Oils. By WILLIAM DAVIDSON, M. D., Glasgow, . 258

VIII. On the Removal of the Bitter Taste and Lichenous Odour of Iceland Moss, &c. By WILLIAM DAVIDSON, M. D. Communicated by the Society of Arts of Scotland, ... 260

IX. Report of the Experimental Committee of the Society of Arts for Scotland, on Dr Davidson's proposed methods of Decolorizing Palm Oil, removing the Bitter Taste and Lichenous Odour of Iceland Moss, and the Fetid Odour of Fish Oils, and for detecting the Adulteration of Fixed Oils. Communicated by the Society of Arts of Scotland, . . 264

X. Some Remarks on Electric Currents in Metalliferous Veins. By ROBERT WERE FOX, Esq. Communicated by the Author, ... 267

XI. Observations on the Digestive Organs of the Infusoria. By J. MEYEN, ... 271

XII. Researches on the Structure of the Scales of Fishes. By Dr L. MANDL. With an illustrative Plate. Continued from page 126 of this volume, . 274

XIII. Remarks occasioned by Dr Mandl's Observations on the Structure of the Scales of Fishes. By M. AGASSIZ, ... 287

XIV. On the best Means of Burning Gas for supplying Heat. By Sir JOHN ROBISON, K.H., Sec. R.S.E., M.S.A., 291

XV. On the best Method of Burning Gas for the purpose of Illumination. By Sir JOHN ROBISON, K. H., Sec. R.S.E., M.S.A., . . . 293

XVI. Abridgment of an Essay on the Construction of Oblique Arches. By EDWARD SANG, M.S.A., Civil Engineer, Edinburgh. Communicated by the Society of Useful Arts for Scotland, . 301

XVII. LORD GRAY'S Meteorological Table for 1839, 319

XVIII. Mean Results, during each Month for Seven Years, of a Register of the Thermometer, kept at Alford, Aberdeenshire; about Lat. 57° 13′ N.; 420 feet above the Sea, and 26 miles inland from the Coast at Aberdeen. Also, the Extremes of both Heat and Cold in each Month, the Mean of each Year, the Mean from April to September both inclusive, and from July to September both inclusive, and the quantity of Rain that fell in the five last Years, with the Fair and Rainy Days. By the Rev. JAMES FARQUHARSON, LL.D., F.R.S., . . 320

XIX. Register of the Weather, kept at Canaan Cottage, Edinburgh, for 1839. By ALEXANDER ADIE, Esq., F.R.S.E., M.W.S., &c., . . . 322

XX. Summary of Observations of the Barometer, Thermometer, &c. for 1839, kept at the Manse of Abbey St Bathan's, Berwickshire, Lat. 55° 52′, W. Long. 2° 23′, at the height of about 450 feet above the level of the Sea. By the Rev. J. WALLACE, A.M. &c., ... 323

XXI. Abstract of Meteorological Observations for 1839, made at Applegarth Manse, Dumfriesshire. By the Rev. W. DUNBAR, . . . 324

XXII. Results of a Rain-Gauge kept at Carlisle. Communicated by Mr JOSEPH ATKINSON, . . 326

XXIII. On the Cultivation of Roses, and the Manufacture of Rose-Water and Attar or Otto, at Ghazeepore, 326

XXIV. Observations on the Temperature of Plants. By M. Van Beck, ... 331

XXV. On the Examination of Mountains and Plains, with the view of discovering previously unknown Repositories of useful Minerals. By the late Frederick Mohs, Councillor of Mines at Vienna, and Knight of the Royal Saxon Order of Civil Merit, &c. 334

XXVI. On the Law of Storms, as communicated to the American Journal of Commerce. By W. C. Redfield, Esq. of New York, ... 361

XXVII. Notes explanatory of some Passages in Professor Keilhau's Paper on Granite, &c., published in vols. xxiv. and xxv. of this Journal, . . . 366

XXVIII. Remarks on Professor Keilhau's Theory of Granite, &c. By Baron Jacob Berzelius, . . 371

XXIX. Presentation of the Wollaston Medal for 1840 to M. Dumont, and one year's proceeds of the Wollaston Fund to Mr James de C. Sowerby, by the Geological Society, ... 379

XXX. Arrangement of Minerals, founded upon Physical and Chemical Characters, ... 383

XXXI. Notice respecting the Fall of an Aerolite, near Juvinas, in the Department of the Ardêche, in France, witnessed on the 15th June 1821. By Professor Forbes. Communicated by the Author, . 385

XXXII. Observations on the Geology of Scandinavia and Spitzbergen. Communicated to the Imperial Academy of Sciences at St Petersburgh, . . 387

XXXIII. Notice of Greenockite, a new Mineral Species of the Order Blende, ... 390

XXXIV. Chemical Examination of Greenockite, or Sulphuret of Cadmium. By Arthur Connell, Esq. F.R.S.E. Communicated by the Author, . . 392

XXXV. New Observations on the Effects of Veins of Quartz. By M. Rozet. Communicated to the Academy of Sciences, ... 395

XXXVI. Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edin-

burgh, and chiefly in the Royal Botanic Garden. By Dr GRAHAM, Professor of Botany, . 397
XXXVII. Proceedings of the Royal Society of Edinburgh, 400
XXXVIII. Proceedings of the Wernerian Natural History Society. Continued from p. 200. . . 407
XXXIX. Proceedings of the Microscopical Society, 414
XL. SCIENTIFIC INTELLIGENCE IN GEOLOGY, MINERALOGY, &c. 416
1. Lantan, a new Metal, ... 416
2. Chemical Composition of Datolite and Botryolite, 417
3. Andalusite and Chiastolite, varieties of the same Species, 417
4. Chemical Composition of Mikrolin, . . 417
5. Chemical Composition of Monazite, . . 417
6. Mineralogical Optical Notice by Babinet, . 418
7. Malthacite, a new Mineral Species, . . 418
8. Serpentine Crystals of Snarum in Norway, . 418
9. Opaline-Allophane, ... 418
10. Davidsonite, ... 419
11. Perikline, ... 419
12. Chloritoide, ... 419
13. Scale for the Fusibility of Minerals, . . 419
14. Recent Deposits of Salt, ... 420
15. Height of the Coast Line of Fuci, . . 420
16. Oxides of Tin and Copper in Spring Water, . 420
17. Analysis of Fossil Bones, . . . 420
18. On the Minute Animals found in Substances during the Process of Putrefaction, . . . 421
XL. New Publications, ... 422
1. Journal of the Asiatic Society of Bengal. Edited by the Acting Secretaries, ... 422
2. Madras Journal of Literature and Science. Number 2d, year 1839, from April to July, . . 422
3. Asiatic Researches, Part II. vol. xx. Calcutta, 1839, 423
4. The Zoology of the Voyage of H. M. S. Beagle, No. I. Part IV. Fish. By the Rev. LEONARD JENYNS, A.M., F.L.S. London: Smith, Elder, and Co., 423
5. Illustrations of the Zoology of Southern Africa. By Dr SMITH. London: Smith, Elder, and Co., . 424
6. Illustrations of the Breeds of the Domestic Animals of the British Islands. By DAVID LOW, Esq., F.R.S.E., Professor of Agriculture in the University of Edinburgh. Part I., ... 424
7. Traité Eléméntaire de Conchyliologie, avec l'application de cette Science a la Géognosie. Par G. P. DESHAYES. Livraisons, 1—5. . . 425
XLI. List of Patents granted for Scotland from 21st December 1839 to 18th March 1840, . 425
INDEX, ... 429

ERRATUM, page 371, last line; *for* 350 *read* 366.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Researches on the Electrical Currents in Metalliferous Veins, made in the Mine "Himmelfahrt," near Freyberg. By Professor F. Reich of Freyberg. Communicated by the Author.

Since Fox first discovered the fact* in copper-veins in Cornwall, it has been known that an electrical current is indicated by Schweigger's Multiplicator, when two points, where ore presents itself, are connected by metallic wire, whether these be in the same or in different veins. Fox repeated the experiments† in lead-veins in other parts of England with similar success. On the other hand, Von Strombeck‡ could find no trace of such electrical currents in lead and copper veins on the right bank of the Rhine. Again, Henwood repeated the experiments in Cornwall,§ and confirmed former results.

I have made similar experiments in the mine "Himmelfahrt" in this district, leading to very decisive positive results.

The method of experiment was in the main that of Fox. When the two points to be connected were determined, a fresh surface was first worked on each, and on this a six inches long, three inches wide, disc of copper, was kept firmly pressed by a wooden strut. An uncovered end of a copper-wire, spun over with silk, was kept pressed to the copper-plate by means of a clamp. The one wire was always short; the other, about 180 metres long, was rolled on a pirn. This latter was retained in all the experiments, the current having thus the same length of wire to pass, so that its influence on the amount of deviation from the needle was constant. The long wire was

* Phil. Trans. 1830, ii. p. 399.

† Transactions Roy. Geol. Soc. of Cornwall, vol. iv. p. 29.

‡ Karsten's Archiv für Bergbau, &c., vol. vi. p. 431.

§ Mining Review, No. x. p. 214, 1837.

let out till it reached to the second point of contact, near which the multiplicator was placed, and the two ends of the fine wires connected with it. The multiplicator, with double needle, and very sensible, belonged to a thermo-electric apparatus of Melloni, made by Oertling of Berlin. In order, in some measure, to judge of its sensibility, it may be mentioned, that the current from a pair of zinc and copper plates, of only one inch square, placed in water very weakly acidified, drove the needle up to the button placed at 90° to prevent further deviation; that an iron-wire connected with two brass-wires placed in the multiplicator, by the mere heating with the hand of the point of contact, produced a deviation of from 10° to 20°, according to the temperature communicated.

When a deviation took place, I tried each time the direction of the current by means of a zinc-copper element, separated by a layer of moistened cloth, and set it down as it was in the long wire of connection. When, therefore, it is said the current is from A towards B, that is as much as to say that at A was a copper, and at B a zinc plate, and the rock or *country* intervening was a moist conductor. In this *country* itself we have, of course, to imagine the current in the opposite direction.

Frequently, also, when no action on the multiplicator ensued, I introduced a zinc-copper element, so that the current must necessarily pass along the wire between the two points of contact. If an action ensued, it was a proof that every thing was in order, that is, that the contact was nowhere interrupted. This experiment is always to be recommended, but especially where no action ensues, in order to have proof that the exception is not the consequence of an interruption in the continuity of the wire, nor of an imperfect contact of the plates with the wire, or with the mineral mass, nor of a deficiency in the adjustment of the multiplicator, nor of any such similar cause. In fact, it once occurred to me, that two points from which, according to previous experiments, an action was expected, induced none. It very soon appeared, however, that the wire was *broken*, and, on its being reunited, the action took place. Some modifications in the method of conducting the experiments will be mentioned, as opportunity presents itself.

The most of these experiments were made in the veins "Frisch Glück," N. and S. vein, and "Neu Hoffnung," NW. and SE. vein. The first of these is a vein of galena, with a north and south direction, and falling 45° towards west. The vein has an average width of 1 metre, and is composed of *galena*, containing $1\frac{1}{4}$ to 4 ounces of silver per hundredweight, in great abundance; *blende* (sulphuret of zinc); iron and arsenical pyrites; and some quartz. The "Neu Hoffnung" belongs to the so-called *spar-formation*, runs from SE. by S. to NE. by N., and falls 68° towards SW. It is composed chiefly of heavy-spar, a little fluor-spar and brown spar, rarely galena sparingly interspersed (eingesprengt), and poor in silver. Where the vein, however, is crossed by others, it contains the richest silver-ores, such as dark and light red silver-ore (rothgiltigerz), silver-glance (glaserz), brittle silver-glance (sproedglaserz) (common melan-glance of Mohs), and radiated pyrites (leberkies). The two veins cross each other at about 190 metres from *Abraham*, main-shaft, and this in such manner, that "Frisch Glück" being passed through and thrown by "Neu Hoffnung," is to be considered as the elder. South-west from this it is partly too little productive to be worked, partly unexplored.

In all, forty-eight experiments were made, which, however, separately to describe, would fatigue the reader. I communicate, therefore, only the results obtained, corresponding as they do almost exactly with those of Fox and Henwood, supporting them with the experiments belonging to them.

1. *Two ore-points separated by a non-metalliferous mass, or between which there occurs a* CROSS VEIN, *or the vein is worked out, give rise to an electrical current in a metallic wire connecting them.*

There were seventeen experiments made, confirming, without exception, the above law. On this account, however, I do not assert that there can be no exception, and that the non-appearance of a current in similar cases, as observed by Fox, and as in the negative results of Strombeck, is to be doubted. It is nevertheless to be desired, that, when the case of the non-appearance of the current presents itself, a zinc-copper

element be introduced, to prove that no defect that might prevent the deviation of the needle exists in any part of the apparatus. The points tried lay either on the same level, and at distances of from 10^{m} to 120^{m} asunder, or the one above the other, at heights varying from 8^{m} to 60^{m}. When the points lay near one another, there intervened either a cross-vein or a shaft; when wide apart, the same was the case, or they were separated by a non-metalliferous mass.

Six times *galena* at a point in "Frisch Glück," was united with points in another vein, viz. twice with silver-ores of Neu Hoffnung, twice with a friable pyrites of the same, once with an isolated eye of galena, in the midst of heavy-spar in this vein, and once with massive fresh pyrites of *Isaac*, N. and S. vein, in the vicinity of Neu Hoffnung, which crosses it. In every case the current went in the direction of "Frisch Gluck," and thus appeared to be positive. The two experiments on silver-ores, consisting of silver-glance, brittle silver-glance, and the dark and light red ores of silver, were made on the 6th drift (about 180 fathoms deep, Tr.), and on different days, but in very similar circumstances. The first time, however, I obtained only 32°, whereas the second 72° to 73° of deviation. This case shews that the deviation may be very different in amount in, on the whole, very similar circumstances, which depends probably less on an inconstancy in the intensity of the current, which cannot be conceived, than on secondary causes, especially on the more or less perfect contact of the disc with the minerals, or of the wire with the disc. The fact is, that in these two cases the latter connection was effected in a different manner for each, and in the case of the greater deviation it was the more perfect. The two experiments on the friable pyrites gave 68° and 47° deviation. In the first case was a short string of pyrites 8 inches wide, in the other case this was only 1 inch. The isolated eye of galena caused a deviation of 26°. The massive pyrites in *Isaac* vein was only of small extent, imbedded in quartz, and caused a deviation of 20°.

I made eleven experiments in one and the same vein, viz. ten on the 6th and 5th drifts of Frisch Glück, N. and S. vein, and one on the 2d drift of Gottlob, E. and W. vein. Eight

times massive galena presented itself at both points, three times only at one, arsenical or iron pyrites or blende being at the other. The *pyrites* and *blende* were, however, in uninterrupted connection with extensive deposits of galena. In the experiments made at nearly the same level, the current went *five* times from S. to N. in Frisch Glück, and *once* from N. to S. In Gottlob, E. and W. vein, from E. to W. In the experiments, where one point lay above the other, the direction was *twice* downwards, and *once* upwards. These results indicate that the direction of the current is quite independent of its position, relatively to the axis of the earth, as well as of the depth, but they are not sufficiently numerous for the complete establishment of the fact.

The deviation generally increased with the distance of the two points, for in Gottlob, E. and W. vein, with the distance 10^m,—a cross-vein causing a *throw* intervening,—the deviation was 23°. In Frisch Glück, with 14^m, worked out space and shafts intervening, 17°, at 50^m 54°, at 64^m 63°, at 68^m 51°, at 70^m 67°, at 88^m 71°, but at 126^m only 36° was obtained, upon which latter we must remark, however, that blende was the mineral at one point, and the small deviation may be attributed to the bad conducting power of this substance. On the other hand, it was under the same circumstances that, at 70^m distance, the needle deviated 67°.

Two galena points at 12^m distance, were connected by means of a wire only 16^m long, and much thicker than the fine one usually employed, upon which the deviation rose from 17° to 25°,—a circumstance proving, that the resistance to conduction encountered by the current in the mass of rock, is not so great that the resistance in the 180^m long, $.04^m$ thick, wire, can be entirely neglected.

In the two experiments between galena and blende, the current was in the direction of the blende, and in those between the galena and pyrites, always in the direction of the galena.

To meet the objection, that the copper discs might have been of different sorts, and that thus an action ensued, the two discs were changed during an experiment, without obtaining the least difference of result. A better proof that the

nature of the discs applied to the ores is indifferent is this:—that, in a connection of galena, of Frisch Glück, and of silver-ores in Neu Hoffnung, at the former point, a zinc disc was laid between the copper and the galena, without the deviation varying in the slightest. Perhaps still more striking is an experiment in Frisch Glück on the 5th drift, when a *galena* point was connected with one of *blende* 38^{m} south from it, without obtaining the least deviation;—probably because the two points were in uninterrupted metallic connection,—which case will be further treated in the sequel. When, in this case, a zinc disc was applied to the *galena* instead of a copper one, there was still no action produced; and here we must expressly mention, that not only the current of a zinc-copper element produced a deviation of 30°, but that also a thermo-electric current, induced by heating one of the stations, caused a deviation of the needle. The cause of this indifference, as to the nature of the metallic disc applied to the ores, is self-evident; for in this case we must consider the currents as produced *by the ores existing in the rock*, whatever may be its cause. *These* are the electrometers, and it is indifferent by means of what metal they are connected with one another, and with the multiplicator. These experiments with zinc discs may also serve to destroy the supposition, that the current observed is only a consequence of the experiment. When the discs are applied to non-metalliferous points, it is then far from indifferent what be the nature of the discs;—of which hereafter.

It might still be imagined that the discs themselves occasioned the current as *thermo-electric*, by a slight difference of temperature at the points of contact having place. The following observations shew that such a supposition is unfounded.

Deviation 18°; temperature 19°.4 C., and 19°.6 C. (about 67° F.) The latter point having been heated to such a degree that the hand could not be held to it, the deviation decreased to 15°.

Deviation 72°; temperature 21°.1 C. (70° F.), and 17°.8 C. (64° F.) The first point having been heated as formerly, no change took place, and the latter having been heated, the deviation was decreased, but almost insensibly.

Deviation 63°; temperature not observed. The disc on galena having been heated, the deviation was somewhat diminished; the other on pyrites being heated, there was an increase to 64°.

The original temperature of the discs was determined by thermometers placed in a sort of case bent from the discs themselves.

These instances prove manifestly, that the great effect by an originally very slight difference of temperature, cannot even be attributed to the same cause as the feeble effect produced, when the discs are strongly heated. When the points connected with the multiplicator belong to the same mass of ore, the heating has totally different effects; to which case we now proceed,—

II. *Two ore-points in uninterrupted metallic connection with one another, induce no electrical current through a wire connecting them.*

Although it is often impossible positively to assert of two ore-points, that within the rock there does not exist an uninterrupted metallic connection, it is, on the other hand, also far from a simple matter to find two points of which it can be said undoubtedly that no fine rent or cross course separates them. Although, therefore, I connected several pairs of points, which were, in all probability, in uninterrupted metallic contact, I can only say so with perfect certainty of two that I chose on the 5th drift of *Frisch Glück*, which were only 4^m apart in massive galena. At first, a deviation of 4° was obtained in the direction from the point *a* towards *b*; *b* was then gently heated, the current instantly changed direction, going from *b* towards *a*, and causing a deviation of 20°. A gently heating of *a* reversed the current again to the direction from *a* towards *b*: mere contact with the hand caused a deviation of 10°. Although the natural temperature of the mass was 20° C. (60° F.), we see from this, how great was the sensibility of the whole apparatus to slight differences of temperature, when the intervening distance was short and the points in uninterrupted metallic contact. Manifestly, the deviation of 4°, observed at the commencement of the experiment, was only a consequence

of a slight difference of temperature, probably induced by the forcible driving up of the strut at the point *a*, whilst the disc at *b* had been in position for some time.

Other cases, in which *probably* uninterrupted metallic connection between two points had place, were,—In Frisch Glück, on the 6th drift, with an interval of 20^{m} without any visible interruption in the continuity of the galena, and where a deviation of 12° was obtained. The heating of that point towards which the current in the wire went, produced a reversion of the same, causing a deviation of 10° in the opposite direction.

In the same vein, two galena points, one 12^{m} above the other,—when a deviation of 7° was observed. The temperature, however, was not noted.

In the same vein, on the 5th drift, with 8^{m} interval of galena interspersed in the gangue, when all action failed, although the current from a zinc-copper element introduced, caused a deviation of 10°, and a zinc-plate having been substituted for the copper one at one point, caused a deviation of 2°.

In the same vein, with 56^{m} interval of massive and interspersed galena, without any action, although the zinc-copper element gave a deviation of 15°.

In the same vein, with 38^{m} interval of massive galena and massive *blende*, without any action, although the zinc-copper element gave 30° deviation,—at the same time, the heating of the one or the other disc produced a current which, strangely enough, in *both* cases, went from galena towards blende, indicating opposite thermo-electric properties of galena and blende in reference to copper.

It deserves also to be mentioned, that, in the above cited case, where in Gottlob, E. and W. vein, two galena points at 10^{m} apart, and disunited by a rent throwing the vein, gave a current through the wire connecting them, causing a deviation of 23°,—the heating of the point from whence the current proceeded increased the deviation ; and that the heating of the other point above it, not only decreased it, but even occasioned a reversion of the current to the opposite direction. This strong action of heat must have been a consequence of the short interval, and of the good conducting power of the

mass within the rock between the two points, although the metallic connection was destroyed by the rent. But the original current could not owe its existence to the quite inconsiderable difference of temperature, as a much greater difference only produced a feeble action.

III. If only one disc be connected with an ore-point, and the other with the timbering, or held in the hand, there is no effect produced on the multiplicator. This to-be-expected result was confirmed several times.

IV. If an ore-point is connected with masses of ore already won, a current sometimes manifests itself, sometimes none. Twice, lumps of galena, piled on the timbering of the sole, and on the lying wall of the vein, were connected with massive galena not yet won in Frisch Glück. In one case, with a horizontal interval of 10^m, no action was obtained, in the other, with an interval of 12^m in the direction of the fall of the vein, 5° deviation was observed, in which latter case too, the zinc-copper element produced a very strong deviation.

V. *When an ore-point is connected with non-metalliferous rock, frequently no current,—frequently, however, a current, though always feeble, yet distinct,—takes place in the connecting wire.*

This result does not correspond with that of Fox and Henwood, who, in the case mentioned, never found a current to have place: perhaps because their multiplicator was less sensible than mine. As these experiments especially excited my interest, I made eighteen of them.

In seven cases, I found no trace of a deviation,—viz. five times by establishing a connection between a massive galena point (where the vein was rich) in Frisch Glück, on the 4th and 5th drifts, with massive *heavy-spar* in *Neu Hoffnung;* once between the same galena point and gneiss in the hanging wall of Neu Hoffnung; and another time between galena in Gottlob vein and gneiss in a cross-cut, being driven to the shaft. Each time a zinc-copper element was introduced, from which a current always passed,—now feeble, now strong, but never so strong as when two ore-points were connected. In two cases, viz. once with heavy-spar, and once with gneiss, a

disc of zinc was substituted for that of copper, and thus each time a deviation, in the one case of 13°, in the other of 5°, was obtained.

In the remaining *eleven* observations, I obtained a deviation of the needle on completing the chain; sometimes, it is true, so feeble, that there rested some uncertainty; sometimes, however, very decided,—viz. by the connection of galena one time with gneiss, 6°; once with heavy-spar, which, however, was somewhat wet, 7°; another time on heavy-spar quite dry, 10°. The direction of the current in the wire was 5 times towards the non-metalliferous point, and 6 times *vice versa*. The zinc-copper element being introduced, always caused a deviation, although very various in amount—one time scarcely observable, and another time amounting to 40°. The greatest intervening distance was in an experiment between galena in Frisch Glück, and brown spar in *Ludwig* N. and S. vein, and amounted to 126^{m}. The multiplicator indicated only 1°; but the zinc-copper element being introduced, gave a deviation of 10°. From these experiments, we learn how easily even a very feeble galvanic current passes through considerable lengths of non-metalliferous rock, which, in hand-specimens, do not allow a trace of much stronger currents to pass. Mr Steinheil had previously pointed out that we can conduct such currents through great distances of ground, and I convinced myself, for short distances at least, of the same fact, as the current from a zinc-copper element runs with considerable force through moist ground in which copper-plates had been placed. This conductibility of substances which, in small specimens, give no passage to galvanic electricity, must be attributed partly to their moist state, partly to their great extent.

Further, these experiments shew, that, when we choose a non-metallic point, it is no longer indifferent what be the nature of the discs applied; for in cases where copper gave no action, a zinc-plate established a current. This, too, is easily explicable.

At the very commencement, the following experiments were made. Massive galena in Frisch Glück was connected with rich silver-ores on the 6th drift of the Neu Hoffnung, and gave 32°. The disc was removed from the galena and fixed

on the gneiss opposite, which was as dry as can be the case in mines where the air is always saturated with humidity. The needle deviated $6\frac{1}{2}°$, and the current continued in the same direction as before. Upon this, the same disc was applied again to gneiss, but that at only 9 inches' distance from the said galena, and the deviation was 18° with the current still in the same direction. When the disc was connected by means of a metallic chain with the galena, the deviation immediately increased. From these experiments, it appears to be a consequence, that, when the one disc is set on a non-metallic point in the vicinity of ore, a current ensues, and this the stronger the less removed the ore is. Now, as we found, on the 6th drift, by connecting a galena point of Frisch Glück with non-metalliferous heavy-spar in Neu Hoffnung, a deviation of 10°, the *Bergmeister*, Mr Fischer, ordered this point to be worked out, and at 0.8^m distance, dark and light red silver-ores, and silver-glance, were found, which otherwise should *never* have been won.

A result so favourable seemed naturally to demand a further research into the matter; still it cannot be denied that several other facts very much decreased the hope of the probability of an application of this means for detecting hidden ores. For example, on the 4th drift, a galena point in *Frisch Glück* was connected with non-metalliferous heavy-spar in the sole of *Neu Hoffnung*, beneath which, at 8 metres' distance, the existence of rich silver-ores was known of, without any trace of an action being obtained, although in the vicinity, upon iron-pyrites, in a cross vein of quartz, 20° deviation was obtained. The distance of the ore must, then, be very short, in order to have any indication of it by a current. On the other hand, I obtained from gneiss, of the adjacent country, in connection with galena, a deviation of 6°; in which case, however, it must be mentioned, that the gneiss was wet, and that at a distance of two metres, a vein containing pyrites crossed. Further, upon heavy-spar, in the vicinity of a string of pyrites, which had caused deviations of 68° and 47°, there was no action.

There remain, consequently, for the solution of this question, so important to the miner, further experiments to be

undertaken; when, at the same time, might be examined, Whether, when two non-metalliferous points are connected, a current ensues? If, with two homogeneous discs, this should never be the case, even when ores are in the vicinity, it must be tried with heterogeneous (zinc and copper) discs, which probably would almost always give a current, and perhaps the more intense when ores were in the vicinity.

VI. Upon the *cause* of the electrical currents observed in metalliferous veins, three different opinions, as far as my knowledge extends, have been put forward. These currents have been attributed to,

a, General electrical currents at the earth's surface, produced either entirely or in part, by the earth's magnetism.

b, Hydro-electric; and

c, Thermo-electric action of the various metallic components of the vein.

The first hypothesis seems to be refuted by the independence of the direction of the currents of their position relative to the earth's axis. Also, on this supposition, two points between which lay a sufficiently conducting medium, even without the presence of ores, should induce a current in a connecting wire. I have convinced myself, however, that two copper-plates, placed in moist ground, and connected by a wire, give rise to no current, although that of a zinc-copper element introduced, is easily transmitted, whether the connecting line of the two discs be in the magnetic meridian, or perpendicular to it.

Thermo-magnetism, which Henwood more especially adopted, I hold incapable of producing such strong currents as have been observed, under the circumstances above detailed, where the chain was generally broken by the intervening of bad conductors, particularly when it is considered that the difference of temperature between the various components of a vein can be but very slight. We learn also from the above experiments, that, by the connection of two ore masses, separated by non-metalliferous rocks, a strong heating of one of the discs effects no alteration in the deviation of the needle of the multiplicator, or at least very small in proportion to the intensity of the current observed. When a metallic connection subsists

between the two points, or when the bad conductors breaking the chain are of small extent, as, for example, merely a cross-course, then, naturally, the heating of one of the discs has a much more decided, or even a preponderating influence.

As, now, the strongest currents were observed exactly where the two points were separated by a non-metallic conductor, there remains only the hydro-electric action of the metallic components of the vein, to be assumed as their main cause. Upon this hypothesis, the non-metalliferous rock represents the moist conductor of the two connected masses of ore forming the electrometer. Thus, when at the contact of the two ore masses with the rock, exactly the same substances do not occur, a current ensues, and this would almost ever be the case. It is also possible, however, that the two ore masses should have one, in every respect the same, metallic component, *e.g.* consisting only of *galena*, in which case, no current could ensue. It is possible that the negative results of Strombeck and Fox originate in this.

In order to prove the correctness of this hypothesis, we must in every case know thoroughly the nature of the mass of ore, and thence be able to predict the result. This is, however, impossible. To contribute something to the subject, I examined, by means of the multiplicator, the series in which the ores, that most generally present themselves in the mass, here follow one another, by dipping them in very dilute sulphuric acid, and found

\+ Zinc,
Copper,
Red silver-ore (not quite free from silver-glance),
Galena,
Silver-glance (brittle and ductile mingled),
Arsenical pyrites,
Copper-pyrites,
— Iron-pyrites,

results varying from those of Pfoff and Ritter. Red silver-ore (Rothgiltigerz) is a very bad conductor. Blende so bad, that no current was obtained from it, and thus its place could not be determined. An application of this series can be made

but very rarely; only when galena is, with certainty, connected with a point consisting of iron and arsenical pyrites, which occurred in four cases, the current in the wire went always from *pyrites* towards galena, in accordance with the above series.

We must not, however, pass in silence a result not very favourable to this hypothesis. When, namely, a non-metalliferous point with one of ore occasions a current, we should look for the cause in the heterogeneity of the copper disc and the metallic components of the ore; as now, from the above, *all* ores are negative referred to copper, the current should go in every case towards the non-metalliferous point. We have seen, however, that of eleven cases this took place only in five, whilst in six cases it was in the opposite direction,—were there, perhaps, ores in the vicinity of the non-metalliferous point, whose intensity exceeded that of galena and copper?

VII. With regard to the extent of the deviation of the multiplicator, it is to be borne in mind, that there can be no immediate conclusion drawn from this as to the electric *difference* of the substance coming into play, for this depends on the resistance to conduction in the entire chain, which again depends on the dimensions and nature of the intervening rock, as also on the more or less perfect contact between the copper disc and the ore, and between the disc and the wire. It is true, as above mentioned, that, from the connection of almost exactly the same two points, I obtained on one occasion 32°, on another 72° of deviation. The cause of this lay probably only in the fact that, in the first case, the wire was attached by merely turning up a corner of the copper disc upon it, while in the other case it was pressed to the disc by a clamp or vice made for the purpose. An idea of how small the power of the electrometers here in action is, may be derived in some measure from the observation, that a zinc-copper element introduced, whose current went in the opposite direction to that observed, surpassed in effect the latter in every case. If the deviation of the needle was great, it was so also on application of the zinc-copper element to the opposite side,—if the deviation was small, then so was that occasioned by the

zinc-copper element; a proof that it is not the intensity of the current from the electrometers, but the resistance to conduction that exerts the chief influence on the degree of the effects observed. This is probably the principal cause, too, why with a non-metalliferous point the effect is never very great; for the resistance to transition from the disc to the bad conducting rock, with proportionally fewer and less perfect points of contact, must be much greater than from a mass of ore still firmly imbedded in the rock even when this is small, but then particularly when it is of great dimensions.

On the Glaciers of the Alps. By M. J. Andre de Luc.*

I have read with interest the notice on glaciers by M. Agassiz, inserted in the preceding number of the Bibliothèque Universelle.† It contains many ingenious ideas, but they require to be confirmed by observations, continued without interruption during a long sojourn in the vicinity of glaciers, and conducted with the same care that Saussure bestowed on his, after which, I had supposed, nothing new of any importance could be brought forward on the subject.

M. Agassiz attributes the progressive movement of glaciers to the dilatation of the water transformed into ice; but the congelation of water cannot take place except near the surface, for, if the glacier be 100 feet in depth,‡ more than nine-tenths of this thickness can undergo no change of temperature, since ice is so bad a conductor of heat, that the water infiltering into its fissures cannot be frozen, whatever may be the season of the year. For this reason, the explanation of the motion of glaciers from the expansion of congealed water, cannot be admitted, with the exception perhaps of three or four feet at the surface, although this effect must be very insignificant;

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* From Bibliothèque Universelle de Geneve, 1839.

† A translation of this appeared in our last Number, p. 383; and a translation of an essay on the same subject, read by M. Agassiz to the Helvetic Natural History Society, was published in vol. xxiv. of this Journal, p. 364. —Ed.

‡ According to Saussure, the Glacier des Bois, in the valley of Chamouni, is from 80 to 100 feet in depth.

and if the ice at the bottom move, it arises from other causes, since it can experience none of the alternations of freezing and melting.

I believe that the movement of a glacier is owing to two principal causes. The first is the pressure of the snows accumulated on its upper parts: these snows are converted into ice, and as at the commencement of a glacier the sides are very steep, this ice or snow exerts a considerable pressure on the glacier, and urges it forwards. This inference may at least be derived from the fact that glaciers advance most regularly in years when a greater quantity of snow has fallen than usual, and when the heat of the summer has been too inconsiderable to melt them.

The second cause of the progressive movement of glaciers, is the continual melting of the ice in that part which rests on the earth, in consequence of the effect of the internal heat of the earth. This melting causes the glacier to sink down, renders it cavernous beneath, and, as it is always inclined, the glacier must necessarily slip forwards. "We often see," says Saussure, "large crevices form in a short time, because the ice being eroded by the waters which flow beneath it, or resting unequally on the irregularly inclined bed which serves as its base, descends and leaves the other masses of ice behind."

M. Agassiz supposes that winter is the season when glaciers are at rest. This, indeed, should result from his theory, but has he assured himself of the fact,—has he inquired of those dwelling in their vicinity? It is possible, on the contrary, that they may move as much in winter as in summer. This conjecture is confirmed by M. Saussure and M. Alb. Haller of Berne. The former states that, in 1764, he had a proof of the motion of glaciers, at a season which may be regarded as winter among the Alps. The latter wrote to me on the 10th of November 1822, that the upper glacier of the Grindelwald had begun to move in 1817, and that the motion had never been discontinued, even in winter, up to the autumn of 1822; and yet in winter the alternations of freezing and thawing are interrupted.

I shall next proceed to extract a few fragments from my notes on the history of glaciers. It is known that from the

year 1817 to 1822, the glaciers of the Alps made further advances than at any preceding period. In 1821, and even as late as June 1822, the Glacier des Bois made a considerable descent, overturning trees, some of which were two feet in diameter, and making so near an approach to habitations that, in the month of June, it was not farther distant from the nearest house than forty paces; on the 8th of June, it was only sixty-six feet distant, and in the month of August sixty-two feet. These facts prove to us that the glacier had never before advanced so far, for no one would have built a house at so small a distance. The intense heat of the summer of 1822 caused the glacier to sink greatly and begin to retire. This effect must have been produced in part by the abundance of water flowing beneath the glacier, and rapidly diminishing its substance.

Let us now turn our attention to the Glacier des Bossons. Previous to the year 1812, the lower extremity of this glacier was surrounded with pines, the size of which indicated that they must have had undisturbed possession of the soil for ages. But in 1812, when a succession of six cold summers commenced, the Glacier des Bossons made successive advances, both in length and breadth. This progress continued till 1818; all the trees were overturned, not only in front of the glacier, but even on its sides. After having destroyed this forest, the glacier covered the meadows, where it was perfectly obvious it had never been before, for neither moraines nor stones had ever existed on these meadows. But in 1820, and particularly in 1822, when the glacier retired considerably, these meadows were left covered with stones, some of which were blocks of great size. It has been observed, in general, that surfaces which have been once occupied by glaciers, lose their vegetable earth and become barren; a circumstance which affords another proof that the meadows of which we speak, and even the site of the forest, had never before been covered by the glacier of the Bossons.

Let us again revert to the upper glacier of the Grindelwald. This glacier advanced in 1817, and continued to do so even in winter, till the autumn of 1822. On the preceding summer, it was more advanced than it had ever been, for, in

1821, it destroyed an old forest, which had existed for two centuries.*

The progress of the Glacier des Bossons, and of the Grindelwald, so similar to that of the Glacier des Bois, proves that the latter had never been so far advanced as it was in 1822, and that Saussure was wrong in taking for moraines of this glacier masses of stones situated at a much greater distance from it than the glacier reached on the year mentioned. M. Agassiz, adopting the opinion of Saussure, reckons seven of these moraines as far as the Bois des Tines; to these he adds ten or eleven others, ascending towards the Col de Balme. It will be seen, in a memoir which I have sent to the Geological Society of France, for insertion in one of their bulletins, that these supposed moraines are rolled boulders, as old as those resting on Mont Salève and on the Jura. I examined them in 1815, when on my way from Chamouni to Argentière.

These are not the only ones which exist in this valley; others are to be found near the Prieuré, likewise near the torrent of Taconay, and on the declivity of a mountain on the right bank of the Arve, opposite the village of Ouches. These latter ascend to the height of 400 or 500 feet above the bed of the river. Huge collections of boulders or rolled blocks of stone are found at certain distances, in all the valleys traversed by the Arve, as far as Mont Salève: they all date from the same epoch.

But let us return to the subject of glaciers. M. Agassiz supposes that the ice, when moving over a rocky ground, sometimes polishes it as perfectly as could be done by the hand of a marble-cutter; it rounds off the angles, scoops out furrows, &c. I greatly question the reality of these effects. M. Agassiz cites as a proof the granites of the Grimsel. I have seen these granite rocks: they present convex and uniform masses; but I am convinced that no glacier ever passed over them. The glacier of the Aar is distant from them more than a league, at least from those in the vicinity of the Hospice. I believe that M. Agassiz derives the facts which he adduces from places where a glacier never passed.

* Letter of M. Alb. Haller of Berne, of November 1822, already cited.

In order to assure ourselves of the effect of a glacier upon its bed, it would be necessary to creep beneath it; for a glacier, when retiring, leaves such masses of stones upon the ground, that it is impossible to discover what goes on beneath; it is supposed that a glacier, by rolling stones under it, may have the effect of rubbing down the rocks.

Our author thus conjectures, that the polished rock of St Bernard has been polished by an ancient glacier. This polished rock is found at the summit of a mountain very remote from glaciers. It consists of the walls of a fissure which penetrate into the mountain at a great angle, and which consequently have never been at the surface; their polish is owing to a quartz coating, in which can be detected the striæ of rock-crystal, or rather this is the effect of a strong friction in the same direction, by the rubbing of one of the walls upon the other.

But I have it in my power to refer to polished rocks where certainly a glacier never passed. M. Thirria,* speaking of the transported matter which fills the depressions and fissures of the ground in the Haute-Marne, says, that this matter is composed of the debris of surface rocks, and these debris fill depressions as well as cavities and rents; now, the bottom of the depressions which these debris fill, and likewise the walls of the cavities and rents which conceal them, *have a smooth and polished surface*, in every respect resembling the caves in the Jura Mountains, which contain portions of diluvial earth along with the animal remains of that epoch.

Such is the cause of the polish on the rocks of Jura in the neighbourhood of Neuchâtel and Bienne, on which M. Agassiz rests his case so strongly. This cause is as remote as the diluvian era, when immense currents carried along great masses of debris. This is likewise the opinion of Professor Studer of Berne.

The object of the memoir which I addressed to the Geological Society of France, is to shew that the phenomenon of rolled or transported blocks of stone is not confined to those which have been observed at great intervals in the central chain of the Alps, but that they are found as far as the base

* Thirria sur le Minerai de Fer de la Haute-Marne. (*Annales des Mines*, 3e serie; t. xv. Paris, 1839.

of this chain, and on its two declivities. This is the case with the chain of Mont Blanc; boulders occur (though in much smaller quantities) on the eastern declivity as well as on the western, and if these have not attracted observation, it is because these collections of stones were taken either for the moraines of ancient glaciers, or for the debris of great convulsions happening in modern times.

P. S.—I confess that I was surprised to observe that M. K. C. de Léonhard, the skilful professor of geology, should have allowed himself to be drawn into error by M. Venetz, chief engineer of the Valais, respecting the cause of the transportation of the boulders observed in the central valleys of the Alps, by taking them for moraines. He expresses himself to the following effect in the French translation of his popular geology (p. 411), published in the present year.

"This force, which has put the masses of rocks found in the valleys and on the summits of the Alps in motion, does not appear to be currents, but rather glaciers, according to the researches of M. Venetz,* a geologist of the Valais; the ice precipitating itself from the region of snows carries along with it masses of rock. The blocks of the Alps are therefore the moraines of ancient glaciers; accumulations similar to those we see in the present day round actually existing glaciers."

It is this opinion which I have ventured to oppose.

On the Laurus Cassia of Linnæus, and the plants producing the Cassia Bark of Commerce. By Robert Wight, M. D.†

The next subject on which I have some remarks to offer is the Laurus Cassia of Linnæus, and the plants producing the Cassia Lignea or Cassia Bark of commerce. My attention was first directed to this subject, by a communication from Government, in which I am requested to endeavour to ascertain "whether the common Cassia Bark of the markets of the

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* For an account of the results of the investigations of M. Venetz, our readers are referred to the 21st vol. of this Journal, p. 210.—Edit.

† From Madras Journal of Literature and Science, No. xxii. January —March 1839.

world, is a thicker and coarser portion of the bark of the genuine cinnamon plant or tree, or whether it is the bark of a plant not analogous to the cinnamon plant or tree."

Before it was possible to return a satisfactory answer to this question, it seemed incumbent on me to ascertain what plant Linnæus meant to designate under the name of *Laurus Cassia*, and whether it was probable the plant so called could supply all the bark passing under that name in the markets of the world. This primary but most difficult inquiry was rendered indispensable by the generally supposed ridiculous assertion of Mr Marshall, that the leaves, and the bark of the trunk and branches of the *Laurus Cassia* of Linnæus, so far from being aromatic and spicy like cinnamon, are bitter, and have in a slight degree the taste and odour of myrrh. This assertion, wide as it may appear of the truth, is yet founded in fact, and what may appear still more extraordinary, has led to a discovery, which, without such aid as he has given, would not probably have soon been made by a professed botanist, a title to which I believe Mr Marshall does not aspire. He appears to have been led to a discovery, simply through the native name, and wonders how it could have received from Linnæus the name of *Cassia*, and had qualities attributed to its bark which it does not in the slightest degree possess. I think I can now answer the question, and explain the mystery which has so long hung on this species, and been hitherto rendered only more obscure by each attempt to bring it to light. It is well known to modern botanists, that many of their earlier predecessors were but indifferent describers of plants, and often very loose in their quotations of figures as synonyms, a sin of which Linnæus was often about as guilty as any of his contemporaries. He seemed to have had an idea, that their figures were generally at best but approximations to the truth, and that if a figure exhibited even a remote similarity to a plant before him, especially if, from the same country, he might with safety quote it as a synonym. Bearing this in mind, we can easily account for a number of errors to which his incorrect synonyms have given rise. The present instance affords an excellent example of what I have here stated, and which, but for the discovery of Mr Mar-

shall, might have long remained undetected. In Herman's herbarium of Ceylon plants, he (Linnæus) found one bearing the native names of "Dawalkurundu Nikadawala," under which, it is referred to, or described in Herman's Musæum Zeylanicum. This he considered a species of *Laurus*, apparently from habit alone, and in his usual brief precise style, calls it, Laurus foliis lanceolatis trinerviis, nervis supra basin unitis; having previously called the true cinnamon, "Laurus foliis ovato-oblongis trinerviis, basi nervis unientibus." The difference between the two, as indicated by the names, seems very slight, merely depending on the one having lanceolate leaves with nerves united above the base; while in the other the leaves are said to be ovate-oblong with the nerves distinct to the base, differences small indeed, and such as could never be found of much avail in distinguishing the one plant from the other, since they are both constantly met with in different leaves on the same tree. Such being the case, it is not much to be wondered at, that botanists should have been surprised by the boldness of Mr Marshall's announcement, that two trees, believed to be of the same genus, and so nearly alike in their external forms, should yet differ so very widely in their properties. But so it is, and nothing can be more certain than that the fact is as he states it.

In proceeding to trace the history of the two species, aided by the light Mr Marshall has thrown on them, our difficulties vanish like mist before the noon-day sun, though Mr M. himself has found it "difficult to conceive how the Dawalkurundu obtained the appellation of *Laurus Cassia* from Linnæus." It was because Linnæus's specimen of Dawalkurundu was neither in flower nor in fruit. Had it been so, he was too acute an observer ever to have confounded it with the plants with which he has associated it in his synonyms. This explanation, it may be answered, is mere assumption on my part: it certainly is so, but supported by so strong circumstantial evidence, as not to leave a doubt of its correctness. Linnæus has, in his Flora Zeylanica, given a short description of each of these species: his description of the cinnamon is principally confined to the flower, and is most precise. In his description of the other, the flower is not once alluded to. Here he de-

clares, that he knows not by what mark to distinguish it from the *Camphorifera japonensium*, which in its foliage it greatly resembles, but nothing can be more distinct than its inflorescence: that of the camphor tree being a panicle, having a stalk as long as the leaves, while in Dawalkurundu, it may be described as a subsessile capitulum, that is, five or six sessile flowers congested on the apex of a very short peduncle, and surrounded by an involucrum of four or five leaves; several of which capitula usually form verticels round the naked parts of the branches where the leaves have fallen.

He begins his description of *Laurus Cassia*,* by stating that he at first considered it a variety of the antecedent (cinnamon), but now that he knows not by what mark to distinguish it from *Camphorifera japonensium*, for the leaves are thinner than those of cinnamon, the nerves uniting above the base as in *Camphorifera*, and are sprinkled beneath with a greyish dew or bloom (*subtus rore cæsio illinita*), as in the camphor-tree, and are at the same time lanceolate, and of a thinner texture than the preceding cinnamon. The whole of his description, in short, agrees most exactly with Mr Marshall's description of the Cingalese Dawalkurundu, and leaves not a doubt that both had the same plant in view, and, consequently, that Mr Marshall is so far correct in saying that bark of the *Laurus Cassia* of Linnæus possessed none of the qualities attributed to it. So far all is clear, but now the chapter of errors begins.

Had Linnæus been permitted to exercise his own unbiassed judgment in this case, it is not improbable he would have avoided the error of assigning to a plant, which, with all his acuteness, he knew not how to distinguish from the camphor-tree, the credit of producing *cassia*, or, at all events, would not have done so without some expression of doubt, so as still to leave the question an open one. But, upon consulting other authorities, he found in Burman's Thesaurus Zeylanicus, the figure of a species of *Cinnamomum* or *Laurus*, as he called the genus, to which Burman had given the name of *Cinnamomum*

* "Hanc speciem olim pro antecedentis varietate habui, nunc vero, qua nota hanc a *Camphorifera japonensium* distinguam, non novi; Folia enim Cinnamomo tenuiora, nervis ante basin coeuntibus ut in camphorifera; subtus rore cæsio illinita, ut Camphora, et simul lanceolata ac tenuiori substantia quam præcedentis."—*Linn. Flor. Zeylanica*, p. 62.

perpetuo-florens, &c., and assigned the native name of Dawalkurundu, not as it appears from the specimen itself having been so named, but because being different from the true cinnamon of which he had seen such specimens and figures, he thought it an inferior wild or jungle sort, what must of necessity, be the plant which Herman had described in his Musæum Zeylanicum, though the inflorescence differed much from the description (a very essential point, which Burman remarked and endeavours to explain away), and therefore gave it the same Cingalese name. Linnæus's specimen not being in flower, and the resemblance between the specimen and figure being in other respects considerable, he had not the means of detecting the discrepancy, and unsuspectingly adopted Burman's figure and name as a synonym to his plant. In Rheede's Hortus Malabaricus (ii. tab. 57), he found the figure of another cinnamon, even more closely resembling his plant in its general aspect than Burman's figure: this he also associated as a synonym, and Rheede's plant being lauded on account of the aromatic properties of its bark and leaves, which resemble the true cinnamon, though it is not the genuine cinnamon tree, he seems to have considered himself quite safe in associating this also, and called the three species, this tria juncta in uno planta, *Laurus Cassia*, and assigned it as the source of the officinal "*Cassia Lignea cortex.*"

After this exposition of the origin of the species *Laurus Cassia*, it can scarcely be a matter of surprise that no two botanists have ever agreed as to the plant which ought to bear the name, nor that not one of them should ever have surmised what plant Linnæus had constituted the type of his species. It is not my intention, on the present occasion, to extend these remarks, by tracing the various conjectures that have been promulgated on the subject; suffice it to say, that no one, so far as I am aware, has taken a similar view as that now explained. It only further remains for me to give some account of the three species thus erroneously associated.

The first mentioned, Dawalkurundu, Linnæus's own plant, and the type of the species, is, I believe, the *Laurus involucrata* of Vahl, and of Lamarck in the Encyclopedie Methodique, and has in Professor Nees' Monograph of the Indian Laurinæ (Wall. Plant. As. rariores) received the name of *Tetradenia*

Zeylanica, but is the *Litsea Zeylanica* of a former work of his —a name which, I presume, must be restored, owing to the other being preoccupied. The slight difference of structure does not seem to render a new genus necessary. The second and third have both been referred, by the same eminent botanist, to his variety of the true cinnamon, the *Cinnamomum Zeylanicum*, a decision to which I cannot subscribe, as I cannot perceive that either of these figures are referable to any form of that species, and they besides differ specifically from each other.

The *Cinnamomum perpetuo-florens* appears to me a perfectly distinct species, very nearly allied to, if not actually identical with, Nees' own species *C. sulphuratum*, of which I have now got specimens from Ceylon. This I infer from the appearance of the plant as represented in the figure; for, if any dependence is to be placed on the description, it is impossible to admit it into the genus. On this, however, I do not feel disposed to place much reliance, as it was not the practice a century ago, when the description was written, to examine the structure of flowers with the same care that is now bestowed. Should it be objected, that the species I quote as the *C. perpetuo-florens* is clothed with yellowish pubescence, which is not mentioned by Burman, then I have another from the same country (Ceylon) perfectly glabrous, agreeing in the form of its leaves, but differing in having more numerous and smaller flowers, which may be substituted, and that I do not think it, more than the other, a variety of the genuine cinnamon-tree.

The Malabar plant Carua (Hort. Mal. i. tab. 57), on the other hand, I consider a very passable figure of a plant, in my herbarium named, by Nees himself, *Cinnamomum iners*; but whether or not I am right in the species to which I have referred it, I can have no hesitation in giving it as my opinion that it is not referable to any form of the *C. Zeylanicum*; neither can I agree with him in thinking the plant figured under the name of *Laurus Cassia* in the Botanical Magazine, No. 1636, is referable to the Ceylon species, but is, I think, very like the Malabar one, the only species of the genus to which the name *Cassia* should be applied, if that name is still to be retained in botanical nomenclature, as being the only one

of the three associated species known to produce that drug. Another plant of the Botanical Magazine (*Laurus Cinnamomum*, No. 2028), I also refer to here, and feel greatly at a loss to account for its introduction into that work under a different name from the preceding. The plant which Nees formerly considered the *Laurus Cassia*, but now calls *Cinnamomum aromaticum*, from China, is a very nearly allied species, but is distinct, and furnishes much of the bark sold in the European markets under the name of Cassia, though it has nothing whatever to do with the *Laurus Cassia* of Linnæus, which, from the preceding history, appears strictly confined to Ceylon and India proper; and that name, not being referable to any one species, ought, unquestionably, to be expunged from botanical nomenclature, its longer continuance there only tending to create confusion and uncertainty. This brings me to the next question, namely, What plant or plants yield the cassia bark of commerce?

The foregoing explanation, in the course of which two plants are referred to as yielding cassia, greatly simplifies the answer to this one.

The first of these is the Malabar Carua, figured by Rheede; the second, Nees' *Cinnamomum aromaticum*. The list, however, of cassia-producing plants is not limited to these two, but I firmly believe extends to nearly every species of the genus. A set of specimens, submitted for my examination, of the trees furnishing cassia on the Malabar coast, presented no fewer than four distinct species, including among them the genuine cinnamon plant, the bark of the older branches of which, it would appear, are exported from that coast as *cassia*. Three or four more species are natives of Ceylon, exclusive of the cinnamon-proper, all of which greatly resemble the cinnamon plant, and in the woods might easily be mistaken for it, and peeled, though the produce might be inferior. Thus we have, from Western India and Ceylon alone, probably not less than six plants producing cassia. Add to these nearly twice as many more species of cinnamomum, the produce of the more eastern states of Asia and the islands of the Eastern Archipelago, all remarkable for their striking family likeness,—all, I believe, endowed with aromatic properties, and probably the greater part, if not the whole, contributing something towards

the general result, and we at once see the impossibility of awarding to any one individual species the credit of being the source whence the *Cassia Lignea* of commerce is derived, and equally the impropriety of applying to any one of them the comprehensive specific appellation of Cassia, since all sorts of cinnamon-like plants, yielding bark of a quality unfit to bear the designation of cinnamon in the market, are passed off as cassia.

Observations on Cinnamon. By Henry Marshall, F.R.S.E., Deputy Inspector-General of Hospitals.

Mr Marshall's paper on Cinnamon, referred to in Dr Wight's remarks, was read at the Royal Society in March 1817, having been communicated by the Right Honourable Sir Joseph Banks; and it was published in the Annals of Philosophy for the months of October and November of the same year. The following is a brief abstract of Mr Marshall's communication, and some new facts, with which he has favoured us.

The cinnamon-tree (*Laurus cinnamomum*) is indigenous in the islands of Ceylon, Sumatra, Borneo, the Sooloo Archipelago, the Nicobar and Philippine Islands, Cochin China, and the Malabar Coast of the Peninsula of India, &c.; and it has been cultivated in Brazil, Guiana, the Isles of Bourbon and Mauritius, the West India Islands, Egypt, &c.

The tree grows to the height of twenty-five or thirty feet, and the stem to a diameter of from twelve to fifteen inches. The young leaves have a scarlet-crimson colour. The bark of the shoots is often beautifully speckled with dark green and light orange colours. The leaves, when full grown, are from six to nine inches long, and from two to three broad. The flowers appear in January and February, and the seeds ripen in June, July, and August. The odour of the flowers resembles the disagreeable smell which emanates from bones when they are sawn. Unless when flowering, the tree emits no smell.

It is alleged that there is a number of varieties of the cinnamon-tree in Ceylon, to which native names have been given; but hitherto it has not been satisfactorily demonstrated that more than one species of the Laurel genus, which yields an aromatic bark, grows there. The distinctive marks of the supposed varieties commonly consist of some slight difference of the shape of the leaves, or of the taste of the bark, neither of which is by any means conclusive evidence of a specific distinction.

Buffaloes, cattle, goats, deer, and horses, eat the leaves, and pigeons and crows devour the berries with great avidity. The tree is disseminated to a great extent, even in the most impassable jungles, by means of these birds, as the germinating quality of the seeds is not destroyed by their stomachs.

There is, perhaps, no part of the world in which the cinnamon-tree

grows in such abundance as in Ceylon, but even in this island it is nearly confined to the south-west quarter. In the other parts of the island it is comparatively rare, and the bark is deficient in the spicy aromatic flavour which it possesses in that which has been called the "Cinnamon country." The cinnamon-tree thrives best in a rich light dry soil, and some degree of shelter from the ardent rays of the sun seems to be beneficial to it. Cinnamon-trees grow, or rather live, in nearly quartz sand, but they yield little cinnamon in this soil, the bark being rough and unhealthy, as is the case in the more sandy parts of the Marendahn plantation, near Colombo.

The *Laurus Cassia* of Linnæus, which abounds in Ceylon, has not an aromatic bark, and consequently this plant does not furnish the Cassia of commerce. It would serve no good purpose to enumerate the botanists who have been mistaken upon this point; Dr Wight having borne ample evidence of the accuracy of Mr Marshall's statement in this respect.

There are four plantations of cinnamon in the vicinity of Colombo, consisting altogether of from eight to ten thousand acres, which afford a large portion of the cinnamon that is exported from the island, but a considerable quantity is also procured from the jungles (natural woods), both in the provinces on the coast, and in the interior or Kandyan country. The principal products of the cinnamon-tree are—

1*st*, *Cassia-buds.* The cassia-bud of commerce is probably the immature fruit, and the fleshy receptacle of the seed, of the cinnamon-tree. The prepared buds have the appearance of nails with roundish heads, and possess the same properties as cinnamon, but in an inferior degree. Cassia, or more properly cinnamon buds, have not hitherto been an article of export from Ceylon, where they might be collected in great quantities. In 1816, Mr Marshall prepared about 100 lb. weight of these buds at Colombo, which were sent home by the late Sir Robert Brownrigg, for the purpose of drawing the attention of Government to this article of commerce, it being quite new, in as far as Ceylon was concerned, for it does not appear that the Dutch prepared them during their occupation of the island.

2*d*, *Cinnamon.* This highly esteemed spice is the prepared bark of the cinnamon-tree. The cinnamon harvest commences in Ceylon early in the month of May, and continues until late in October. Shoots having a diameter of from half an inch to three inches, yield better cinnamon than either smaller or larger shoots or branches. The shoots are peeled by making two or three longitudinal incisions through the bark, and then introducing a knife under the bark, and thereby separating it from the wood.

The green cuticle, or outer bark, is scraped off from the inner, which, after being carefully dried, becomes the cinnamon of commerce. The Ceylon cinnamon is commonly formed by the union of pieces of bark while still soft, into quills or pipes about forty inches in length. Great care is taken to prevent the cinnamon which is exported from being mixed with the bark of other trees, or with injured bark of cinnamon shoots. There is considerable difference in the quality of cinnamon, which is probably occasioned by varieties in the climate, soil, or exposure in which the plant grows, the age and health of the tree, and partly

to the care and skill employed in its preparation. As to climate, it may be observed, that cinnamon which grows to the northward of Putlam, or to the eastward of Matura, is greatly deficient of aromatic flavour, and rarely considered fit for the market. Cinnamon is exported from Ceylon in bales of 92½ lb. weight, covered with double cloth made of hemp, not, as has been stated, of cloth made of the bark of the coco-nut tree, which has no bark; there is no pepper or coffee introduced into the bales, as has been alleged.

From the time the English took possession of Ceylon, until about 1823, the East India Company had a monopoly of the cinnamon produced in the island. For the purpose of superintending the sorting and baling of the cinnamon, the Company employed an inspector and two assistants at Colombo, and for a few years Mr Marshall was one of the assistants. The cinnamon was divided by the sorters into three qualities, a first and second sort, and a third or rejected kind, which is generally in small pieces or chips. The Company's contract comprehended the first two sorts only, the third or rejected sort remaining in possession of the Ceylon Government. It was part of the agreement between the contracting parties, that the third sort should not be imported into Europe, which would have reduced the value of the first and second qualities in the market. For a number of years no use was made of the rejected cinnamon, by which means the storehouses became lumbered to such a degree, that General Maitland, while Governor of the island, directed a large quantity of it to be burned.

The following abstract will shew the quantity of the 1st and 2d sorts of cinnamon which was exported on account of the East India Company from Ceylon for a period of about sixteen years.

Years.	When Shipped.	Number of Bales.	Number of Pounds.	Nature of the Contract.
1802	Nov. 1802	1401	282,217½	Under contract by Mr Dundas at 3s. a pound, and profit beyond 5 per cent. to be accounted for to the Ceylon Government.
1803	May 1803	1650		
1804	March 1804	3769	348,632½	
1805	March 1805	3274	302,845	
1806	Feb. 1806	2559	236,707½	
1807	Feb. 1807	3271	302,567½	Under contract by Mr Windham at 2s. 8d. a pound, no profit to be accounted for.
1808	Feb. 1808	4865	450,012½	
1809	Feb. 1809	4012	371,110	
1810	Feb. 1810	3910	361,675	By letter from the Earl of Liverpool under date 5th March 1810. The old contract concluded by Mr Dundas in 1801 being revived.
1811	March 1811	4435	410,237½	
1812	March 1812	4000	370,000	
1813	March 1813	4600	425,500	
1814	Feb. 1814	2965	274,262½	
1815	— 1815	4178	386,465	Under contract made in May 1815 by Earl Bathurst at 4s. 6d. a pound, for a quantity not exceeding 450,000 pounds.
1816	— 1816	4251	393,217½	
1817	Feb. 1817	3700	342,250	
	— —	1344	124,320	

The Honourable the East India Company continued to contract for the Ceylon cinnamon until about the year 1823, from which period it was remitted to the Colonial agent in London to be sold, and the proceeds placed to the credit of the funds of the colony. Shortly after Sir Robert Brownrigg became Governor of Ceylon, some use was made of the third or rejected sort of cinnamon, as will appear by the following statement.

Statement shewing the quantity of rejected cinnamon which was disposed of by the Ceylon Government in 1815 *and* 1816, *and the amount of revenue derived from it.*

Years.	Quantities.	Price.	Amount.
			L. s. d.
1815	1,785 lb.	10 fanams per pound	1,586 16 9
1816	139,828 ...	ditto	12,429 ... ...
——	82,099 ...	75 rupees per 100 pound	7,381 18 7½
——	39,006 ...	2s. 6d. per pound	4,875 15 ...

The above quantity of cinnamon was sold to Madras and Bengal merchants, they engaging not to transmit it to Europe, but which, it was universally believed, reached England under the denomination of *cassia*, not of cinnamon. No cinnamon has been exported from Ceylon, as cassia, since the contract with the East India Company terminated.

3*d*. *The essential Oil of Cinnamon.*—This oil is prepared in Ceylon, and generally from the broken portions which are separated from the quills during the inspection and sorting. The cinnamon chips are grossly powdered and immersed for about 48 hours in sea water. When the process of distillation commences an oil comes over which separates into two kinds, a heavier and a lighter. The light oil separates from the water in a few hours, but the heavy oil requires ten or twelve days to separate completely. Eighty pounds weight of cinnamon yield about two and a half ounces of light oil, and five and a half ounces of heavy oil. The light and the heavy oil are kept distinct and sold separately; they are never mixed. The following extract from an advertisement in the Colombo Journal will shew the market value of the different kinds of cinnamon-oil.

Report of the sale of cinnamon and clove oil held in October last:—

Heavy cinnamon-oil, . 29 lb. 12 oz. at 3s. per oz.
Light cinnamon-oil, . 29 ... 12 ... at 3s. ...
Clove-oil, . . . 76 ... 7 ... at 4d. ...

The following quantities will be put up for sale on the 7th January:—

Heavy cinnamon-oil, . . . 18 quarts
Light cinnamon-oil, . . . 18 ...
Clove-oil, ... 45 ...

Colombo, December 18. 1832.

Portions of bark which have been deprived of their volatile oil, are never substituted for cinnamon, as has been frequently asserted.

4th, Clove-oil.—The mature leaves of the cinnamon-tree yield, by distillation, an essential oil, which greatly resembles the oil of cloves, and for which it may perhaps be substituted.

5th, Camphor.—This substance may be obtained from the bark of the roots of the *Laurus cinnamomum;* but the roots of the *Laurus camphora,* which it is said grows in Ceylon, yield it in a much greater quantity.

By decoction, the ripe berries yield an inodorous substance resembling suet, which is seldom extracted, and rarely applied to any useful purpose.

Until the year 1833, the preparation and trade of cinnamon in Ceylon was monopolized by Government. All the trees in the colony were public property; and persons who were discovered uprooting trees, or cutting cinnamon, even in their own grounds, were liable to the penalty of transportation. But since the 9th March 1833, "all restrictions and prohibitions against the cultivation, possession, or sale of cinnamon by private individuals," have ceased; and consequently, any person may now plant cinnamon trees on his own grounds, prepare the bark, and vend or export the produce at pleasure, upon paying the requisite export duties.

Cassia.—We are still, it appears, much in the dark with regard to the tree or trees which supply the aromatic bark of commerce usually denominated Cassia, and it is sincerely hoped Dr Wight will direct his attention to the investigation of that interesting subject, being a highly important one, not only in a botanical, but in a commercial point of view. The cassia of commerce, which is chiefly imported from Canton, differs from Ceylon cinnamon, inasmuch as it is made up into small parcels of two or three pounds weight, about eighteen inches long, and is usually imported in boxes. Neither the plant which produces Canton cassia, nor the place where it grows, seem to be specifically known.

The words *cinnamon* and *cassia* were, very early in the history of mankind, employed to designate either two different substances or different qualities of the produce of the same tree. They are both mentioned in Exod. xxx. 23, 25.

The cinnamon-tree is a native of a tropical climate, and the prepared bark was probably conveyed to Palestine from the places where it grew, by means of Phœnician merchants (Genesis xxxvii. 25, and Ezek. xxvii. 19). Herodotus informs us, that the word *kinnamon* was adopted by the Greeks from the Phœnicians, and in all likelihood the Hebrew term *kinnemon* or *kanam* has a similar origin. The country which produces an article of commerce, very generally gives it the name which it obtains in other parts of the world; hence we must look to the language of a country which produces cinnamon for the origin of the terms that are employed to designate it by consumers. In the Malay language, cinnamon is designated by the words *kayu manis* (sweet wood), from which the Hebrew and Greek names of this spice may have been derived, the cinnamon-tree being found in great abundance in the Malay islands. *Kannema,* signifying sweet wood, is the Malabar name of this spice. In the Persian language it is called *kinnamon,* and in some parts of India it is known by the ap-

pellation of *dar Chinie*, which signifies the wood of China. Cinnamon or cassia was for a long time imported into Europe under the name of "China wood." The Malay word *kayu* (wood) seems to have been the origin of the Hebrew word *kiddah*, which is translated *cassia*, and the Latin term, by which this aromatic bark is known in commerce, is *Cassia lignea*. In ancient times the unpeeled shoots or branches were conveyed to Europe, and sold wood and bark together; and hence, in all probability, is the origin of the adjunct "lignea." Moses was directed (Exodus xxx. 23, 25) to take of myrrh, *sweet* cinnamon, sweet calamus, cassia, and olive-oil, certain quantities, and thereof to "make an oil of holy ointment, an ointment compound after the art of the apothecary: it shall be an holy anointing oil." How was the art of the apothecary exercised in preparing the holy ointment or oil? Perhaps it was prepared by a process similar to that which the natives of India have from time immemorial practised to prepare odoriferous oils. The aromatic substances employed are coarsely powdered, and put into an earthen vessel along with a specific quantity of fixed oil. Water, sufficient to cover the aromatics, is then added, and the vessel placed upon a fire. During the process of ebullition, the essential oil of the aromatics unites with the fixed oil, by which means it is impregnated with the peculiar odour of the seeds, barks, or other substances employed.

Cinnamon is mentioned in the Song of Solomon, chap. iv., and in Proverbs vii. 17; and cassia in Ezekiel xxvii. 19. The "sweet cane," mentioned in Isaiah xliii. 24, and Jeremiah vi. 20, is in all probability only another designation of cinnamon. That it was not the sugar cane may be inferred from the verse last quoted, where it is described as coming "from a far country." The Crusaders found the sugar cane in great abundance in Syria, which they denominated *Canna Meles* (honeyed reeds); and, consequently, we may conclude that it was indigenous in Palestine, although sugar seems not to have been manufactured from the cane earlier than the fifth century.

On the Transverse Valleys or Openings in Chains of Mountains which afford a Passage to Rivers. By J. André de Luc.

The phenomenon of transverse valleys or openings, which afford a passage to rivers through mountain-chains, is met with in the four quarters of the globe, and even very frequently; but it cannot have been sufficiently attended to, since a late traveller, when speaking of two Indian rivers which cross the first chain of the Himmalaya by a deep opening, says,—"This singular course has nothing else analogous to it in the other chains we met with, where, in general, the currents belonging

to opposite declivities do not cut across the axis of the principal chains." *

The fact is, that this peculiarity,—undoubtedly a very marked one,—is met with in many mountain-chains in all the four quarters of the world.

To commence with Europe.—Our first example shall be the Rhone, in the course it follows, to pass from the plain of Geneva into that of Lyons. It first crosses the passage of l'Ecluse, which is an opening separating the first range of the Jura from the mountain Vuache; then it turns suddenly westward, and escapes from the first range, when it recovers its southward direction at Bellegarde. Between Culles and Chanaz, it crosses the second range without changing its direction; but at Pierre-Châtel, it turns suddenly to the west, in order to traverse an appendage of the second range; it then resumes its southern direction as far as St Genis d'Aoste. Here the river describes an acute angle, and flows to the north-east, to cross the third range between Grolée and St Sorlin, and to escape entirely from the chain of the Jura. This place is described by M. Jules Itier, in his manuscript memoir on the asphaltic rocks of the chain of Jura. Amongst the numerous and vast fractures which cut across that chain, "I shall cite," he says, "the transverse valley which the Rhone follows in its course from Cordon to St Sorlin; it is evidently the result of a displacement which has formed, on the right bank of the Rhone, the escarpements of Glandieux at St Sorlin, and on the left bank, those of Quirieux at Vertrieux, as well as the narrow gorge of St Alban, in which the Rhone scarcely attains the breadth of 75 metres."

Our readers will peruse with interest, I doubt not, the reflections made by Addison on the same subject, in 1702, in his *Remarks on several parts of Italy, Switzerland, and the Tyrol*, page 309.† "As I have seen a great part of the course of this river, I cannot but think it has been guided by the particular hand of Providence. It rises in the very heart of

* Bibliotheque Univ. de Genève, No. 39. Mars 1839, p. 127.

† Remarks on several parts of Italy, &c. 12mo. Hague 1718.

the Alps, and has a long valley, that seems hewn out on purpose to give its waters a passage amidst so many rocks and mountains that are on all sides of it. This brings it almost in a direct line to Geneva. It would there overflow all the country, were there not one particular cleft that divides a vast circuit of mountains, and conveys it off to Lyons. From Lyons there is another great rent, which runs across the whole country in almost another straight line, and, notwithstanding the vast height of the mountains that rise about it, gives it the shortest course it can take to fall into the sea. Had such a river as this been left to itself, to have found its way out from among the Alps, whatever windings it had made, it must have formed several little seas, and have laid many countries under water before it had come to the end of its course."

Similar reflections may be made on the Elbe at its departure from Bohemia. This river finds a deep passage across the mountains which separate that country from Saxony. This passage is a valley of surprising variety of aspects; it intersects an entire chain of mountains of very variable height. The following are a few details respecting this passage, derived from the geological travels of De Luc in Germany.

At Aussig, De Luc remarked in a first range of mountains a transverse opening, which afforded a passage to the Elbe, and, after issuing from thence, the river all at once changed its course nearly at a right angle, to follow the direction of another valley. De Luc descended the Elbe from Tetschen to Pirna in a boat. The course of the river serpentines between steep rocks, the correspondence of which presents all the characters of a double fracture; sometimes the strata of these rocks descend towards the river, sometimes they incline in an opposite direction. The mountains which border the river are intersected by many valleys or *combes*, the sides of which are abrupt. In one place the passage of the Elbe appears coompletely closed up; in front of the observer rises a steep mountain, but the river soon turns to the right, and traverses one gap, then a second, and finally issues from this chain of mountains altogether.

As we approach Pirna, the rocks on both sides become lower, their declivities gentler, and some of them are covered

with vineyards, which descend almost to the margin of the river; the latter is of great breadth at this place, and flows very slowly. Such is likewise the case during its course across the mountains; it is remarkable for its slowness, and for the smoothness of its surface, which resembles a mirror. "The calmness of the river along which I sailed," says De Luc, "whose surface was without a ripple, allowed me to enjoy the delicious rural scenes presented to my view. It was the finest season of the year, when the villagers were employed in collecting their second crop of hay, and the fruits of their orchards. The pastures were covered with cattle, goats browsed on the rocks, and the good housewives, with their children around them, were engaged in cleaning their kitchen utensils, or in washing the vessels belonging to their dairies." De Luc was pleased to think that these happy villagers' course of life was as tranquil as that of the river from which he had so much satisfaction in observing them.

M. Alex. Brongniart, in his Natural History of Water, remarks, that the Rhine, after leaving the Lake of Constance, traverses a chain of mountains distinctly characterized, which run from the south-west to the north-east; it forms, on the left bank, the northern extremity of the chain of the Jura, of which Mont Terrible forms a part, and on the right bank the mountains of the Forêt Noire. The Rhine crosses this chain almost perpendicularly to its direction; its bed is bordered and obstructed by rocks, and its course is extremely impetuous. Arrived at Bâle, it changes its direction, and flows tranquilly, and without obstruction, through the wide valley of Alsace; but, after passing Mayence, alters its route, and directs itself against a chain of mountains which seems to bar its passage. This chain is known, on the left bank, by the name of Eifel: it is a continuation of the Ardennes; and on the right bank, it takes the name of Westerwald. The Rhine enters it at Bingen, and crosses it, not by a valley, properly so called, but by a gorge, which admits only the passage of the river: it emerges from it near Coblentz.*

Let us now turn our attention to the Danube. This river is

* The distance from Bingen to Coblentz is about 13 leagues.

very smooth in Hungary, because flowing through a very flat country; but among the mountains of Bannat and Servia, where it is considerably confined, it acquires a fearful rapidity, which, in connection with the shoals occurring here and there, renders its navigation extremely dangerous.

M. Ami Boué states that the first narrow passage of the Danube is a rent which cuts a mountain across. Lower, the river runs through a second contraction, where its bed presents a series of rocks and rapids. At the first defile, the banks of the river are so steep that there would be much difficulty in making a road; the road at present passes over the mountain. These narrow passages are called Les Portes de Fer. M. Lippy, a naturalist quoted by D'Aubuisson, was struck with the correspondence of the beds on the different sides of the passage; all appeared to him to indicate the former junction of the two chains now separated by these Portes.

Mr Lyell, in his Principles of Geology, remarks, that in the south-east portion of England, the draining off of the waters is not effected by rivers which follow the great valleys excavated in the argillaceous beds, but by valleys which run in a transverse direction, passing across the chalk to enter the basin of the Thames on the one side, and the Channel on the other. In this manner, the chain of chalk hills in the north is cut asunder by the rivers Wey, Mole, Darent, Medway, and Stour; and those of the south, by the Arun, Adur, Ouse, and Cuckmere.* If these transverse openings were filled up, all these rivers would be forced to turn to the east, and flow into the sea, in the counties of Sussex and Kent.

Asia.—In western Asia, the country of Diarbekir presents us with an example similar to that of the Elbe, of a river flowing from a girdle of mountains by a narrow defile. Diarbekir is a country of an elliptical form, surrounded by mountains, and intersected by upwards of eight rivers, which unite in the Tigris. This river issues by a long defile, bordered with very high rocks, which do not admit of a passage for travel-

* Considering the limited extent of the country, these rivers, compared with those of Europe, may be regarded as mere rivulets.

lers along the banks. In this place, the army of 10,000 Greeks, under the command of Xenophon, were arrested in their march along the banks of the Tigris, and obliged to pass the Cardusian mountains on the right, in order to cross the elevated plain of Armenia, and reach the shores of the Black Sea.

Among the Siberian rivers, the Upper Irtisch has its sources in the great Altai mountains; it flows into the lake Oz-Nor-Zaïsan (Lat. 47° N.), into which five other rivers discharge themselves. The Irtisch again issues towards the north, traverses a chain of mountains, the eastern part of which bears the name of the Lesser Altai, and the western that of the mountains of Beszka.

The river Yenisei likewise crosses the continuation of the same chain of mountains more to the east, and passes between the Lesser Altai mountains and those of Sayansk. We thus perceive that the two rivers named have a part of their sources on the northern margin of the great plain of Central Asia, and cut asunder the mountain-chain which forms its reverse; their base is contracted between steep banks.

According to the great map of Asia by Arrowsmith, the Indus or Seind has its sources in Little Thibet, some of them coming from the north-west, others from the south-east. These streams unite, in order to pass together the chain of mountains which border India on the north. In the place where the Indus crosses it, it bears the name of the Indian Caucasus on the north-west, and the country of Cashmire on the south-east.

The Sutlej (Setledje) crosses this same chain of mountains more to the south-east, where it bears the name of Himmalaya. The Sutlej then turns towards the W.S.W. on its way to join the Indus. When crossing the mountains, it flows very near the sources of the Ganges.

Three of the rivers of Nepaul cross the chain of the Himmalaya, and, consequently, their sources are beyond it; they discharge themselves into the Ganges.

The Brahmaputra, whose mouths are very near those of the Ganges, has a very different course. Its sources are on the northern declivity of the Himmalaya Mountains, being separated from those of the Ganges only by this mountain range.

It flows towards the east, inclining a little to the south, throughout the space of 15 degrees of longitude, or 325 leagues. The river then turns suddenly to the south, then towards the south-west, where the chain of mountains, which is a continuation of the Himmalaya, terminates. It afterwards again turns to the south, in order to double a more southern chain of mountains, and proceeds to throw its waters into the Bay of Bengal.

Africa.—The traveller Burchell mentions six openings in mountain-chains which run across the southern extremity of Africa. I shall confine myself to a notice of the two principal ones. The Dutch at the Cape of Good Hope call them *Kloof*, from a word which signifies rent or fissure. Two of them are observed in one of the mountain-chains, through one of which the river Breede flows, and the river Hex through the other.

The Kloof of the Hex is a sinuous defile, on each side of which mountains rise in majestic forms, clothed with a profuse vegetation. The strata of these mountains appear to have been disturbed; they are often inclined at an angle of 45 degrees. The Kloof is particularly picturesque. Its steep sides re-echo the rattling of chariots and the cries of their drivers. Large trees, ornamented with rich foliage, soften the harsh features of the towering rocks, which sometimes shoot forward in enormous masses, sometimes retire, and form the most umbrageous and profound retreats. On issuing from the Kloof, the traveller enters the valley of the Hex, which is long and narrow, and surrounded on all sides with mountains.

The *Roodezands Kloof*, or Red-sand Kloof, is a narrow winding defile, about three miles long, affording space only for the passage of the small river Berg, on each side of which the mountains rise abruptly to a great height. The rocks are covered with bushes and trees from the summit of the mountains to the very edge of the water, presenting a superb and highly picturesque view, ornamented with every description of foliage. .

North America.—It may be said that in the Apalaches, of which the Alleghanies form the central or principal chain, nearly all the great rivers cross the lateral chains and the lon-

gitudinal valleys, as well on the north-west side as on the south-east. M. Brongniart names two on the north-west, and five on the south-east, which are so circumstanced. I shall mention only the three last.

The Susquehanna, which descends from the very crest of the Alleghany chain, cuts the Blue Mountains at their middle, in Pennsylvania.

The Potomack, which forms the boundary between Maryland and Virginia, follows precisely the same route; it intersects the same chain as the Susquehanna, and at this place another river joins it, in order to pass together through the same crevice.

The river James, which traverses Virginia, and likewise descends from the highest ridge of the Alleghanies, intersects the mountains of the north, and those of the south, which are chains of the second and third rank, and likewise two other longitudinal ridges occurring between the latter chain and the sea-coast.

In the States of Carolina and Tennessee, the river Tennessee crosses three mountain-chains, that of Coweta, the Wuaka, and the Chilhowe mountain. Other rivers join the Tennessee, namely, the French-broad, and a second, both of which cross the Pecko-Grant's chain, which forms a continuation of the Smoky Mountains. After the junction of all these tributaries, the Tennessee traverses three ranges of mountains, named the Lookout Mountains.

The geological map of Georgia, of North Carolina, and of eastern Tennessee, by Jacob Peck, shews these passages of the rivers very distinctly; one would be disposed to say that the mountains had been cut across for the express purpose of affording an outlet for their waters.

The American Journal of the Arts and Sciences, vol. xix. pp. 6 and 7, contains an account of a voyage along the river Lehigh in Pennsylvania, and of a passage across a defile in a chain of mountains called Blue-ridge, of which the following is an extract:—

"As we approached the gap, the view became very beautiful, and as we entered it by the side of the Lehigh and of the fine canal upon its left bank, the mountain-ridge, here cleft

from top to bottom, and rising apparently a thousand feet, presente, on either hand, a promontory of rocks and forests, rising very abruptly, and forming a combination both grand and beautiful. The passes of rivers through mountains are almost invariably picturesque, and it is always interesting to observe how faithfully the rivers explore the clefts in mountain barriers, and, impelled by the power of gravity, wind their way through rocky defiles and pursue their continuing course to the ocean. It is common to speak of such passes as being formed by the rivers, which are often supposed to have burst the barriers, and thus to have shaped their own channel. This may have happened in some peculiar cases, and there are doubtless many instances where the lakes, of which many must have been left at the retiring both of the primeval and of the diluvial ocean, have worn or burst away their barriers, especially when composed, as they must often have been, of loose materials. But, with respect to most rocky passes of rivers through mountains, there appears no reason whatever to believe that the waters have torn asunder the solid strata; a more resistless energy must have been requisite for such an effect, and we must therefore conclude that the rivers have, in most instances, merely flowed on through the lowest and least obstructed passages; their channels they have doubtless deepened and modified, often to an astonishing degree, but they have rarely formed them through solid rocks. Soon after passing Lehighton, a little village three miles from Mauch Chunk, we entered another pass, which the Lehigh makes through mountains. It is not like the former, a section of the barrier; it is rather a long circuitous gorge between two barriers, which, although they pursue a winding course, still preserve their parallelism; and their feet, near Mauch Chunk, approach so near to each other, that there is only room for the Lehigh and the canal on one side, and for a road cut into the mountain on the other; it is so narrow that the river is almost within reach on the right, and the mountain-rocks are quite so on the left."

South America.—The Orinoco, at St Fernando de Atabapo, turns all at once to the north, and passes a chain of

mountains by running through a fissure. In this part of its course, it forms the great cataracts of Maypuris and Aturès; its bed is confined between enormous masses of rocks, and, as it were, divided into different reservoirs by natural dykes. After passing Carichana, it flows languidly through a plain to the Ocean.

From the lake of Lauricocha to St Jean de Bracameros, the Maragnon, or river Amazon, follows a longitudinal valley, nearly parallel with the ridge of the Andes, and, after a course of more than 200 leagues towards the north, it turns to the east, in order to avail itself of a passage between two mountains; it then cuts all the lateral branches of the Andes, by running between two nearly perpendicular walls of rock, and finally issuing in the direction of Borja by a very narrow pass called the Pongo de Manseriche from the name of an adjoining country. The Pongo, at its narrowest part, is twenty-five toises broad, and two leagues in length from the place where the contraction commences as far as the town of Borja. M. de la Condamine, who descended the river in a boat, estimated that it ran at the rate of two toises (12 feet) in a second.

We have now mentioned numerous examples of rivers flowing across mountain-chains by passages excavated, as it were, expressly to afford them an outlet; insomuch, that this procedure is far from being confined to the Indus and Setledje, but is so often met with that it may even be regarded as a general phenomenon, by no means to be ascribed to accident, but to an evident design.

I shall terminate this note by transcribing the following reflections which I find among my numerous manuscripts.

There are strong reasons for believing that chance has not presided over the arrangement of mountains and valleys, hills and plains, seas and continents; neither are we to ascribe to that cause the fixing of the level of the ocean relatively to the land. If it were so, how could it have happened that this distribution should have been so marvellously adapted to the existence of the innumerable organised beings, animal as well as vegetable, which people the earth? Who does not perceive in this the superintendence of a great intelligent Being, who,

having created these beings, wished to preserve their existence, so that they all might live and multiply? He has not left to chance the care of fashioning the surface of the earth, to the introduction of irregularity and confusion. The same All-Powerful who has given existence to organized beings, must have directed the causes calculated to give to the surface of the earth a new form, to render it most fit for securing their preservation, and to present them with all the objects tending to accomplish their well-being.

On Rivers which break through Mountain-Chains. By the late Professor Hoffmann of Berlin.

The late Professor Friedrich Hoffmann, in his "*Physikalische Geographie*" (which forms part of his "*Hinterlassene Werke*"), gives the following summary of examples of rivers which traverse mountain-chains.

This occurrence takes place in almost all the larger rivers of the globe, and in some of them repeatedly; and there is hardly any important chain of mountains which is not, at some point, thus cut through, transversely to its line of bearing. The greatest range of mountains in the world, the Himmalaya, is broken across by the rival of the Ganges, the Brahmaputra; and, leaving out of view the narrow rents which occur in the higher parts of the mountains, almost all the streams which quit the Alps issue from transverse fissures, termed in many places gates or passes *(Pforten)*. Thus, the Rhone, where it leaves the Alps, cuts through one of the highest chains before it enters the lake of Geneva; it finds its way, in a narrow gorge, between Martigny and St Maurice, through the Gate of the Canton Valais *(die Pforte des Wallis)*, passing between the Dent de Midi and the Dent de Morcles, which rise to a height of at least 8000 feet above its level. The same takes place on the other side of the Alps with the Adige or Etsch, near Chiusa and Roveredo. Thus also does the Inn issue from a narrow ravine between Kufstein and Rosenheim; and so, in the defiles of Golling and Lofer, the Salza and the Saal burst through the chain of the Salzburg Alps, which rise be-

twixt them to a height of 8000 feet in the lofty summit of the Watzmann.

No country in Europe, so far as we know, presents so many instances of this phenomenon as Spain. Ritter remarks (*Erdkunde*, vol. i. p. 70, 2d edition), that, on this account, and because the division of the country into provinces is founded on the water-sheds, our maps generally convey the most erroneous idea of the direction of the mountain-chains; for the water-sheds occur chiefly on plateaus, from which run branches of mountains.

Germany exhibits many cases of this most important hydrographical feature. The ***Erzgebirge*** is cut by the Elbe right across its line of direction, where it is connected with the Riesengebirge by the hills of Upper Lusatia *(Ober-Lausitz)*, viz. between Tetschen and the neighbourhood of Pirna, or at that portion of the river which traverses the chief part of the beautiful rocky landscape generally known by the name of the Saxon Switzerland. The Weser, ere it reaches the great plain, cuts across a range of hills *(the Wiehengebirge)* from 600 to 800 feet in height, at the *Porta Westphalica*, near Minden; and the Ems, on a smaller scale, traverses the continuation of one of these chains of hills at Rheine. Nowhere, in Northern Germany, is there a more instructive instance of the breaking through a mountain-chain, than in the rocky gorge of the Ross-trappe, through which the river Bode quits the northern edge of the Hartz Mountains.

But no German river is so remarkable for striking and repeated examples of this feature as the Rhine.* Where it leaves the Alps, it cuts through them, by one of the deepest and narrowest gorges that they present, viz. the *Schamser Thal*, from Splügen downwards, and its continuation to the vicinity of Hohenembs. It then crosses the wide valley between the Alps and Jura, expanding itself in the basin of the Lake of Constance, but next penetrates the opposite wall of the Jura; and the most considerable ridges are indicated by

* See Professor Weiss's paper, "Uber die Rhein-Durchbrüche;" in the *Zeitschrift für die neueste Geschichte, Staats-und Völkerkunde*, Berlin, April 1814, p. 363.

the falls at Schaffhausen, and the rapids at Laufenburg on the southern edge of the Black Forest. At Bâle it has left this mountainous wall behind it, and now bends its course between two parallel mountain-ranges, which accompany it quite according to the old ideas on the subject; these are on the one side the Black Forest and the Odenwald, and on the other the Vosges and the Hardt. Suddenly, however, this broad valley is blocked up by an immense barrier at Bingen and Mayence, for the slate mountains of the lower Rhine cross the course of the river by the Taunus and Hundsrück mountains, which form a steep wall of 2000 feet in height. The mighty stream enters a narrow rocky gateway at Bingen; and the margins of the mountain mass which has been broken through and torn asunder to its very base, accompany it onwards, the phenomenon being continued for a space of between fifty and sixty English miles till near the town of Bonn. There the mountains gradually diminish in height, or retire, and the river then pursues its uninterrupted course through the plain, probably burdened with the debris which it has carried away from the mountains. Nowhere can the arrangement of the watersheds, and the inequalities of the land, be more opposed to the old view than here; for the stream receives the numerous tributaries from the borders of the mountains, and these tributaries flow into their interior. How much the opposite would the course of the mountains be, to what we observe in nature, were we to form our idea of them from the arrangement of the net of rivers given on the map.

It is also well worthy of attention, that, even in the lower parts of Germany, where the inequalities of the surface are not so well marked, the rivers have broken through the highest portions of the ridges. This is the case with the Oder below Frankfort, and the Elbe near Hitzacker. We thus perceive, that everywhere the same causes have been in operation, and the same effects have been produced.

Abstract of a Paper read to the Society of Arts, by Sir John Robison, entitled, "A Narrative of Experiments and Suggestions by the late Mr James Taylor, for the Application of Steam to Navigation;" with Introductory Remarks by Sir John Graham Dalyell, President of the Society. Communicated by the Society of Arts for Scotland.*

THERE has been too much cause of complaint, that the true benefactors of mankind being denied their merited reward during life, it is reserved for posterity, by acts of tardy justice, to honour their memory.

No example is more notorious than in the subject of the following paragraph.

Of all modern inventions, that affording the highest practical utility, is navigation by the power of steam;—one which seems to bid defiance to the impediments of time and distance,—which, in conquering the elements, conspires to human safety.

Yet not only was the author of this notable contrivance suffered to languish in obscurity,—to die unnoticed and unknown; but his widow and daughter have been hitherto overpassed in any adequate remuneration.

But we are no strangers to national rewards for useful projects. They have been conferred of late, for improving the mode of inoculation in one department, and for improving the highways of the kingdom in another. Or the widows and children of those who have fallen in the public service, are recompensed with honours and with bounties, descending even to the second and third generation. Ought not the nearest and only relatives of the author of so important a discovery, now justly reclaimed by Scotland, to enjoy some due proportion of public favour?

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* For very full details regarding the connection of the late Mr Miller of Dalswinton and Mr Taylor with the early history of steam-navigation, we refer our readers to the Memoirs of Patrick Miller, Esq., printed in the Edinburgh Philosophical Journal, vol. xiii. p. 81, and the Edinburgh New Philosophical Journal, vol. iii. p. 87.—EDITOR.

14th November 1839.

In obedience to the request made to me yesterday at the Meeting of the Society of Arts, that I should draw up a short abstract of the paper then read by me, containing "a narrative of experiments and suggestions by the late Mr James Taylor (of Cumnock), for the application of steam to navigation," I now beg leave to forward you the following notes.

In the year 1785, the late Mr Miller of Dalswinton was engaged in speculations on the practicability of propelling vessels by paddle-wheels; and, in 1787, he constructed a double boat of 60 feet long, with paddle-wheels in the space between the two vessels: these were worked by capstans turned by men. The speed of this vessel was tried against a row-boat belonging to the Custom-House, which was distanced in the race.

The men at the capstans having had to make exertions which could not be maintained for any long period, Mr Miller became desirous of substituting some other mechanical power for that of the men, and consulted Mr Taylor (at that time the tutor of his sons) regarding it. On this, Mr Taylor proposed *that a steam-engine should be applied to give motion to the paddle-wheels.* After making some objections, Mr Miller consented to be at the expense of an experiment, and authorized Mr Taylor to employ a clever mechanician, of the name of Symington, to make a small engine with a 4-inch cylinder. This was accordingly done, and on the 14th October 1788, this engine having been erected on a twin-boat, *the first steam-boat voyage ever made,** was successfully performed on Dalswinton Lake.

Encouraged by the striking success of this trial, Mr Miller agreed to make another on a larger scale, in consequence of which Mr Taylor, and his engineer Mr Symington, proceeded to Carron Foundry, where an engine with a cylinder of 18 inches' diameter was prepared and fitted to a vessel, which was tried on the Forth and Clyde Canal, for the first time, in November 1789. This vessel moved at the rate of seven miles

* A claim has lately been made to the credit of this application of steam-power, by M. Jouffroy, in 1775 or 1776, on the Saône in France.—J. R.

per hour,—a rate which has hardly been exceeded by the most improved modern steamers, in canal waters.

The expense of this experiment having exceeded the estimates, Mr Miller became dissatisfied, and declined to proceed any farther, nor could he ever be induced to resume the subject.

In 1801, Lord Dundas, then Governor of the Forth and Clyde Canal Company, employed Mr Symington to make an engine for an experimental vessel for that company. The vessel was soon after completed, and made many experimental trips on the canal,—but being found to create a wave destructive to the banks, was on that account laid aside.

It was this latter vessel (the third in succession from the first trial), which was visited and studied by Mr Fulton, the American engineer, whose first steam-boat was launched in 1807, nineteen years after the successful trial on Dalswinton Lake. Mr Henry Bell of Helensburgh, who constructed the steam-boat "Comet," on the river Clyde in 1812, accompanied Mr Fulton in his visits to Lord Dundas's vessel.

The above noted facts are supported by vouchers from various persons, several of whom are still alive, and are fully corroborated by accounts published in the Dumfries Journal and the Scots Magazine of 1788, and in the Edinburgh newspapers of February 1790.—I remain, Sir, your very obedient Servant, JOHN ROBISON.

James Tod, Esq. *Sec. Soc. of Arts.*

It is not generally known that the engines fitted in Fulton's first steamer, on the Hudson, were furnished by Messrs Boulton, Watt, and Co. of Soho, who were applied to by Mr Fulton, in person, to undertake to construct them.

On the Habits of Testaceous Mollusca belonging to the genera Achatina and Phasianella. By F. LOGAN, Esq., Surgeon, R. N. Communicated by the Author.

THE following facts and observations, collected from notes taken at the Isles de Loss and Rio Nunios in 1830, relate to some natural phenomena seen in two species of testaceous mollusca, belonging to the genus Achatina, and in another belonging to the genus Phasianella. The Isles de Loss, about

seven degrees from the equator, and four miles from the African coast, are three in number, at least only three are of sufficient importance to deserve notice in this place. They lie in the shape of a horse-shoe, forming inside a beautiful harbour. On these islands two species of that beautiful genus of shells named Achatina were found in great plenty: in shape and size they were nearly the same, but in colouring very different. The full-grown shell was four inches long, and one and a half wide at the mouth: such as seemed to have completed only the first stage of their existence, were not more than one inch in length. The phenomenon that excited my curiosity, and led to the ascertainment of the following facts, was that of finding many of them in a torpid or dormant state. It is known almost to every one, that, in northern latitudes, the Helices, and I believe, all the shelled mollusca of a terrestrial kind, after having placed themselves, at the approach of winter, under proper shelter, such as old walls, holes in rocks, beneath stones, &c., make an earthy or calcareous operculum which exactly fits the mouth of the shell, after which the animal becomes torpid or dormant. This condition is emphatically called Hybernation. Until the above facts presented themselves to me, I supposed (and every thing that I had read or heard on the subject, tended to confirm the very plausible opinion), that cold was the only cause of torpidity in these animals, and that nature had provided them with a power of forming this crust to protect them from the cold, and the intrusion of insects or vermes, which might prove fatal to the animal during its state of dormancy. In these latitudes, however, within seven degrees of the equator, they have no winter to provide against, nor cold to produce torpidity; yet here, as in Europe, they are to be found in an inactive state, and seemingly benumbed, shut up in their shells and covered over with a pure white, firm, calcareous operculum, which is nicely fitted to the mouth of the shell, and fastened with a mucous or gluey substance. This operculum has no attachment to the animal; but after a short immersion in warm water, falls off, leaving the mouth of the shell smooth and entire.

These observations having established one fact, that cold could not be the cause of torpidity, at least amongst the mol-

lusca of these climates, my observations might have ceased here, had there not been found in the same places, viz. in the holes of rocks and trees, and under loose stones, amongst those individuals, provided with an operculum, and in a dormant condition, one or two others without an operculum, and seemingly in a state of activity. This was on the week before Christmas, the warmest season of the year on this coast; and it being doubtful to me whether these last-mentioned kinds had not just come into a hiding place for the purpose of hibernating like those beside them,—or whether there could be any particular season of the year which might be the cause of this disposition to a change of state, I was induced to make inquiry of some of the natives, many of whom can speak a little English, as to where the animals could be seen feeding. One man made me understand that they never came out of their hiding-places till after sunset, and that they could only be seen by carrying a light. Accordingly, at night a lantern was procured, and the person alluded to engaged as a guide. After a short search my guide said, "me tink om lib here," pointing at the same time to two small shining bodies, diverging from each other until their two extremities at the summit were about two and a half inches apart, and at the lower ends apparently not more than half an inch. Each seemed about the thickness of a small quill, and their power of reflecting the light was so great, that they had exactly the appearance of two small polished bars of silver. These were the two long tentacula of the animal, and, but for them, we might have sought in vain. The animals were found feeding on the tender shoots and leaves of luxuriant plants, generally near the roots of decayed trees, or at the bottom of shady rocks, where vegetation acquires additional vigour, and where the animal also finds an easy retreat in the day time. After having collected a sufficient number in all their different stages of growth, and made a comparison of their different sizes, I found that, to complete one of the largest, required six new growths or augmentations; and on examining such as were operculated and in a dormant state, the same number of augmentations could easily be perceived. I likewise found them operculated and torpid in all the six different stages of advancement, with the tip of the shell always firm

and complete. On the contrary, of those that were feeding and active, some had nearly completed, others little more than commenced, their intended new addition; in fact, they were found in many shades of advancement in regard to the new augmentations to their house, the lip of the new piece being in general so soft that it yielded to the slightest touch of the finger. On removing the calcareous crust from those dormant and operculated, they shewed little or no signs of life, except when excited by pricking or otherwise, when they would slowly withdraw themselves back into the shell. On leaving them to repose, they very slowly returned to the former position. Such as were active and feeding, shewed great excitability; the slightest touch made them rush into their shell with a gurgling noise: but, when left to themselves, they soon returned and shewed their tentacula, which the others never did.

Lamarck has occasioned some doubt respecting the branchial organization of the animals in this genus, by conjecturing that they may breathe either water or air: surely he does not mean the one and the other alternately. That many of the species of this genus feed upon the banks of rivers and stagnant water is very true, but there exists no proof of their ever breathing water, since we find many belonging to other genera whose principal, and some whose constant, abode is in that element, breathing nothing but air. If he had taken the trouble to examine for himself, no room for conjecture would have been left, as the animal in question is only provided with low reticulated branchial cordonets, not standing up in tufts, filets, plates, pectens, or vascular knobs,—evidently exposing too small a vascular surface for the action of water on the fluids. Besides, none of the animals of this genus that I have ever seen are provided with an operculum; a fact that Lamarck very justly sets out with at the commencement of his arrangement of land and fresh-water shells, as being a certain proof that they only breathe free air. As to the localities where the mollusca in question are found feeding and dormant, we may mention that they are a cluster of small islands, in general very precipitous and craggy, with rocky and steep shores, or huge blocks of stones piled upon one another. The

whole of the islands are without either stagnant or running water, yet there is a plentiful supply of fresh water for the inhabitants, as well as for shipping. The springs which supply it issue from below the shelving or perpendicular rocks, some of which are about high-water mark, and many of them considerably below it, so that in some places fresh water cannot be obtained till half ebb. Many of the animals above mentioned were found so far up amongst the rocks and crags, that two weeks would be required, even at a good snail's pace, to enable them to reach the water; and if they did venture in, the first sea would dash them to pieces amongst the rocks. More has been said upon this subject than some may think necessary; but error, when propagated by a man like Lamarck, ought not to be slightly passed over, as it becomes doubly dangerous.

At the end of March and beginning of April, three months after the above remarks were made, in the entrance of the Rio Nunios, I found upon the roots, trunks, branches, and leaves of many of the mangrove trees, a subspecies of the Lamarckian genus Phasianella in great numbers. They are furnished with a coriaceous opereulum, and breathe only water. Those found upon the trunks and roots below high-water mark, were not adhering to the tree more firmly than our *Turbo littoreus*, and like it, when pulled off, the animals made a sudden retreat into their shell, at the same time discharging the greater part of the water contained in their bronchiæ. But those on the branches and leaves of the trees above high-water mark, were found adhering much more firmly; they were also observed to discharge much less water from the bronchiæ,—some of them very little, others almost none, requiring considerable pressure with the finger to make it at all perceptible. This led me into a more minute examination, and I soon found that the greater part of those on the leaves and branches above high-water mark, were completely glued to the bark of the trees or leaves by a sort of white shining gum or mucus, so as not only to exclude insects, but also the atmospheric air. Many of them adhered so firmly, that the lip of the shell would break off and remain adhering to the tree or leaf, and in all cases a crackling or tearing noise was made on

pulling them off. The animals seem to be provided with this gum in great abundance, for a shining white streak marks their course wherever they go, so that, by getting upon an elevated place, the trees which they inhabit may easily be distinguished. They seem to live in societies, it being rare to fall in with a solitary straggler, and only inhabit a tree here and there.

On farther prosecuting these observations, I found that those glued to a tree or to leaves, as above stated, like the operculated Achatina, shewed little or no signs of life till pricked or otherwise excited; when they would retreat into their shell, with a slow and almost imperceptible motion; some of them, in the same slow manner, gradually discharging a quantity of water; in others, the quantity was scarcely discernible. The latter, I supposed, must have nearly completed their period of repose, as they could not live long after the water was completely exhausted in their bronchiæ.

I had a great desire to mark the exact duration of time these animals remain in this dormant state, but my stay in one place was always too short to enable me to accomplish my purpose. From the facts above stated, it would appear that the mollusca contained in the shell just mentioned, at certain natural periods, with which neither climate nor season have any concern, go into a dormant and inactive state; being provided, at the same time, with the power of shutting up the mouths of their shells, either with a hard calcareous operculum, or by gluing them to other bodies with a strong gum. Without this precaution they would be devoured, in their dormant state, in a few hours, by the ants which swarm everywhere, being observed in myriads even on the tops of the mangrove trees, where the tides rise and fall from thirteen to fourteen feet, and where, in many places, they are at least half a mile from dry land. The first time I observed these mollusca on the tops of the mangrove trees, made me naturally suppose that they were phytophagous, and that they frequented the leaves for the purpose of feeding; but, after examining the leaves with great care, no trace of gnawing or eating could be seen, even where they were in greatest

numbers. It was therefore impossible for me to say whether they were zoophagous or phytophagous.

It appears to me, that the above facts lead to the following inquiries:—

1*st*, Has the period been ascertained at which any of the great family of mollusca begin to exercise their reproductive functions?

2*d*, Do they all pass certain periods of torpidity before arriving at their full growth?

3*d*, Do they reproduce after each period of rest, or not till they have arrived at their full growth, or nearly attained to it?

4*th*, After arriving at full growth, how often do they reproduce before they die?

5*th*, May it not be supposed that, during each of those periods of rest, certain changes are produced in their organization, which may answer some such end as the pupa state amongst insects; or at least may effect the purpose of consolidating and perfecting the last new growth which has been hastily sketched out during the feeding period?

The determination of the length of the periods during which these animals remain in a state of rest or activity, would be highly interesting, and might be easily accomplished by any one residing for a year, or perhaps half that time, in a tropical climate, as there is little doubt that all animals of the same kind and genera have similar habits in the same latitudes throughout the world.

Zoological Instructions of the Royal Academy of Sciences, Paris, for the Scientific Expedition repairing to the North of Europe. Drawn up by M. Isidore Geoffroy Saint Hilaire.

Since the year 1732, the date of Linnæus's celebrated journey through Lapland, the Scandinavian Peninsula has been many times explored, almost in every part, by distinguished zoologists, or at least skilful collectors. Notwithstanding the zeal of the naturalists belonging to the new Commission, and the favourable circumstances in which they are placed by the

friendly concurrence of the Swedish Government, we cannot, therefore, expect that the discovery of a great number of objects new to science will reward their efforts. But neither subjects of research, nor the means of being useful to zoology, will be wanting to them in the various countries which they are successively to traverse.

In the Scandinavian Peninsula the transition gradually takes place of the Fauna of temperate and central Europe, with which we are so well acquainted, into the fauna of the circumpolar regions, respecting which, on the contrary, we possess very imperfect information. In no other place—and it is this which appears to us to constitute the principal interest of the study of the zoology of Scandinavia—can this transition be observed and traced with so much advantage to science. The north of Russia, the only European country which, with Lapland, extends to the north of the Arctic polar circle, may undoubtedly hereafter afford equal facilities, but cannot at present supply grounds of comparison of equal interest for zoological geography. This possibility cannot be realized, till the Fauna of temperate Russia, by becoming as well known to us as that of central Europe, shall afford a perfectly established term of comparison for the Fauna of the Arctic regions of the Russian Empire.

By placing the subject in the point of view just indicated, the zoologists of the expedition will give the greatest scope to their researches, and confer most benefit on science. Researches thus planned, ought, in fact, to be directed to a triple object, viz. to render the history of our species of central Europe complete, in its various relations; to collect as ample materials as possible for the history of the Arctic species, which is often incomplete, and occasionally altogether unknown; finally, to bring together all the facts fitted to throw light on their geographical distribution, and on the relations they bear to each other in the Scandinavian Peninsula.

§ I. Of these three objects, thus generally expressed, but which we shall successively revert to, and decompose, so to speak, into their principal elements, the first, in our opinion, is neither the least difficult nor the least important. At a period not very remote from the present time, it might have

appeared singular, earnestly to recommend zoologists about to visit a distant country, carefully to study there the animals of their own country. It would, then, have been supposed that researches were attended with no useful object when they went merely to the establishment of a few slight differences, and the addition of a few species more to the long series of animals already known. The progress of science has, happily, modified, and continues more and more to modify, this tendency in minds endued with a very imperfect understanding of the admirable work of Linnæus; and *varieties of locality*, those *demi-species*, as they have elsewhere been called by one of us,—*sub-species*, as they are named by German authors, now excite the interest of all distinguished zoologists as much as the groups termed species themselves.*

This is not the place to discuss, or even to indicate the scientific controversies for which the difficult subject of varieties of locality has afforded a text; still less can we examine how far we may be permitted to hope that these minute, and sometimes almost insensible, differences, so long neglected, may one day afford a key to the greatest and most strongly marked. It is enough that important questions affecting the very philosophy of science, have been started regarding the *varieties of locality;* it is enough that the solution of the difficulties which now beset zoology, and tend to make its progress uncertain and vacillating, depends on the deep study of these varieties; it is enough, in short, that there exists any doubt on the subject, to authorize us to demand of the zoologists of the expedition, observations and collections which Scandinavia affords them such a favourable field for making. This vast peninsula, presenting at the same time extensive plains and great chains of mountains, lies sufficiently near us to render it likely that nearly all our species occur there; but at the same time it is sufficiently distant, and, in particular, sufficiently different in its climate, to make them most frequently exhibit very notable modifications. The small number of facts now known to us, enables us to foresee the interesting results which may be obtained by a comparison of the Faunas of the two countries, when it shall be founded on a sufficient

* See Note A, at the end of the Zoological Instructions.

number of examples selected from all the degrees of the zoological scale.

Travellers who traverse a country more or less rapidly, or who, at most, remain for a few months at a time in the same locality, as it forms part of the plan of the new Commission to do, cannot evidently, of themselves, collect all the materials necessary for the comparison which we have pointed out. But, besides what they can accomplish themselves, the members of the expedition cannot fail to find most valuable aids in many places. There are at present in Sweden, Norway, and Denmark, many people who study either the whole subject of zoology, or some one of its branches, scientifically, or as mere amateurs, and who are forming rich collections of the productions of the country. Among the Swedish zoologists, M. Nilsson, in particular, whose courtesy is equal to his knowledge, may prove very useful to the Commission, in consequence of the unceasing efforts he has made for many years, to collect and bring to greater completion the necessary materials for his Scandinavian Fauna.

While urging the zoologists of the expedition to collect all the objects they can for the purposes of comparison, which will be the more satisfactory the broader the basis on which it is founded, we may at the same time point out a few zoological groups which appear to us to be of greatest interest in this point of view. Such are the lepidoptera; terrestrial mollusca; lacustrine and river mollusca and fishes; birds of prey; passerine birds, and, among these, sparrows in particular; gnawers; but, above all, the beaver, whose habits in Scandinavia ought to be determined with care;* and the fur-bearing carnivora, especially those of the genus mustela. These latter will possess a double interest, as they can be compared with the martins, pole-cats, and ermines of North America, as well as with our own.

It is almost unnecessary to remark, that these materials cannot be of full value to science, if, along with each animal, there is not transmitted as exact an account as possible of the geographical situation, and topographical disposition, of the locality in which it was taken. The season of its capture ought

* See Note B.

also to be mentioned. These circumstances, always so useful, become indispensable when we wish to compare individuals of the same species, but from different localities; for the slighter the differences we attempt to appreciate, the more complete and precise ought to be the means of comparison.

The zoologists of the expedition may likewise co-operate very usefully towards the completion of the history of European species, by collecting, by every means in their power, information regarding the migrations, whether of fishes, littoral birds, or land birds of the interior of the Scandinavian peninsula, more particularly in its northern part. Many travellers, especially Acerbi, according to Julin d'Uléaborg, and some other modern zoologists, have already published various documents on these subjects, but many still remain to be collected. It is to be desired that a list could be drawn up, as complete as possible, of all the migratory species; and that we should be able to answer the four following questions in regard to each of them.

1*st*, Do its migrations take place regularly every year?

2*d*, What is the period of its arrival?

3*d*, How far does it advance northward?

4*th*, What is the date of its departure?

If, as there is every reason to think, these questions cannot be answered in regard to all the species, it will be at least useful if the zoologists of the expedition attempt to give a precise solution for a certain number of them, taken as examples. We may point out, in particular, for their attention, the swallow and cuckoo, a bird of which it will be otherwise desirable to study the manners with care, in order to see if it does not differ, in this respect, from the cuckoos of central Europe, as we are assured, but without sufficient proof, to be the case with those of Japan.

In regard to the animals which do not migrate, it will be of very great interest, at least in relation to the northern part of the peninsula, to collect all the facts calculated to make us understand the condition in which they pass the winter. We especially recommend this consideration among the higher animals, in respect to the gnawers, insectivorous species, badgers, and bears themselves, about which it will be easy to

obtain new information in Scandinavia, as it unfortunately produces too great a number of them. The same consideration applies with still greater interest to the small number of reptiles existing in Scandinavia, fresh-water fishes, and all animals of the inferior classes, whether aquatic or terrestrial. Lastly, most people are acquainted with the celebrated but doubtful observations of Olaüs Magnus, Archbishop of Upsal, according to which we ought here to add swallows, or at least one of the species, the bank-swallow. Pretty numerous observations, which, according to some authors, should have confirmed those related by Olaüs, are far from being sufficient proofs of so paradoxical a fact as the hybernation of swallows would be; but they have created doubts in some enlightened minds, and therefore we must invite the zoologists of the expedition to collect information on the subject in the different parts of Scandinavia which they shall successively visit, and hold an inquest, if we may so speak, on this question.*

The freezing of a part of the fluid contained in animals more or less completely deprived of life by severe cold, and the possibility of reviving them, as they do naturally in spring, by the slow and gradual restoration of heat, are indisputable facts, but still imperfectly known. Experiments undertaken by one of us,† but left incomplete, demonstrate them in the most positive manner, but they do not go beyond that. The members of the expedition will, no doubt, find themselves very favourably circumstanced to extend, and ultimately complete, our knowledge of these curious facts. It will be useful that they verify them, not only on animals placed by them in circumstances fitted to produce congelation, but, what your reporter has never had an opportunity of doing, found frozen in the very circumstances under which they naturally live. We especially recommend the observers who take up this subject, to determine exactly, in the first place, the relative position of the minute particles of ice found under the skin; in the second place, the state of the blood contained in the heart and larger vessels; finally, the degree of the animals insensibility. All these observations are evidently possible under

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* See Note C. † See Note D.

the climate of Paris, and even in much more southern parts of Europe; but they will be made with greater ease, and be at the same time more complete, inasmuch as the circumstances can be more varied in the cold regions, where many of the naturalists are to be detained during the approaching winter in their zeal for science.

Finally, and to conclude what we have to say regarding our first general question, the domestic animals, usually so neglected by travellers, ought to attract the attention of the zoologists of the expedition, as well as the wild ones. The knowledge of the first concerns us perhaps in a different sense, but without doubt it concerns us as much as that of the second. It is to be desired that specimens could be brought home of all the small or middle-sized races, particularly the dogs, sheep, and fowls. The possession of the skeleton is, in this case, as essential as that of the skin. With regard to the very large kinds, oxen, for example, the preparation and carriage of which would be attended with too great difficulty, correct drawings made on the spot, and the transmission of the crania and horns, would be sufficient, particularly if an exact account be added to these of the number of vertebræ and ribs. In every instance, and in regard to all the races, it will be very interesting to collect all possible information on the advantage derived from them, on the mode of rearing and the care bestowed on them; and, above all, on the duration of their growth, and the age when they attain the adult state, and become fitted for continued labour and reproduction. Finally, such notices as can be obtained respecting the period of their importation, and the original native country of some of the races, will form a very useful addition.

§ II. We shall have less to say regarding the animals which specially compose the Arctic Fauna; not by any means because the wants of science are here fewer, but because they are more generally felt, and, moreover, much more easily pointed out.

A very great number of the animals of the circumpolar regions have been described, noticed, or figured in catalogues; but the greater part have not been examined in their native

haunts by the zoologists of central Europe ; and the history even of such of them as are best known, for example, the rein-deer, elk, polar bear, the glutton, and lemming, still presents numerous desiderata.

Of all the animals of Lapland, the rein-deer is the one which ought most to attract, and which, in fact, has really obtained the attention of travellers ; but, from that very circumstance, its history has become obscured by doubts and fables without number. It will be easy for the members of the expedition to solve the one, and dissipate the other, by collecting all the materials for a faithful relation of this animal's manners, and of all the circumstances which render it so useful to the Laplanders, and so remarkable to zoologists. The points which it appears to us it would be most useful to illustrate are,—the effects which castration produces on the males, particularly in regard to their antlers, effects which appear to vary according to the period and circumstances in which castration is performed ; the state of the female's horns, apparently as variable ; finally, the difference in the races which may exist of this species. It will be useful to obtain the skins and crania of the fawns at different ages, as well as a male and female of the wild rein-deer, taken in Spitzbergen or in one of the circles of Lapland, where the race passes for being most completely free from any intermixture with the domesticated rein-deer.

The young of the elk are still rarer in collections than those of the rein-deer, and the possession of the adult male is of itself desirable, as well as precise information respecting the greatest size it attains, and the variety in the form and size of its antlers.

It is the young also that we have most to desire in regard to the polar bear. The comparison of them with the young of other species, cannot fail to give rise to some interesting remarks. All that has been related of this formidable species, and of its ferocity in spring, it will be of great advantage to subject to revision.

We likewise have details, in part contradictory, transmitted to us by travellers on the habits of the glutton. Many even of the facts generally admitted regarding it are not sufficient-

ly authentic. Finally, it is also desirable that there should be obtained a series of the different varieties of age in this animal, or at least a male and female, with one of the young.

The migrations of the lemming, their irregularity, every thing referring to the circumstances in which they take place, ought also to become the subject of a new examination, and too much care cannot be bestowed upon it. The more interesting these facts are to science, the more necessary it is to subject them to a rigorous examination. We particularly recommend to the zoologists of the expedition to make new observations on the direction of their migrations, which, according to authors most worthy of confidence, sometimes take place, on the approach of winter, from south to north; for example, in 1742, a year in which the winter was most rigorous in the more southern provinces. It is owing to these facts, as is well known, that the lemmings have the credit of enjoying the faculty, to us absolutely incomprehensible, of foreseeing severe winters; an instinct which has, in like manner, been ascribed to other gnawers, and to a great number of migratory birds.

Among the mammifera, we have further to recommend, and with the more interest because, in this case, it is not only information regarding the animals, but the animals themselves, that we want, or of which we scarcely know anything,—1*st*, The narwhal, whose singular and gigantic weapons of defence are so common in collections, and of which we even possess a few broken crania, but whose skin (unless it may perhaps exist in an old collection of natural history at Hamburgh) and skeleton, are wanting in all the museums of Europe. Either the one or the other of these is one of the finest objects which the expedition could procure. In want of them, the possession of the viscera, a well preserved cranium along with a tusk, crania, although broken, with two tusks, would be of great benefit to science. The narwhal does not appear to be rare in the Icy Sea between Greenland, Spitzbergen, and the North Cape; that is to say, precisely that part of the Arctic ocean which the expedition will traverse, if it repair to Spitzbergen; and, it may be added, the animal is stranded at times on the coasts of these different countries. The expedition will, doubt-

less, neglect nothing to procure so precious an animal, and thus fill one of the most-to-be-regretted gaps in European museums. But if our zoologists cannot succeed in this, we recommend them to collect, principally from the fishermen they may find at Spitzbergen, the fullest and most precise information on the manners of this cetacea, especially on the combats in which it is said to engage with the whale, its kind of food, the size of the oldest males, that of the females and young, the maximum length which their tusks attain to, their condition in the female and young, the degree of rarity of the species with two tusks, and finally, the different disposition of the single tusk in the ordinary individuals.

2*d*, Whales, cachalots, and, in general, all the cetacea of the Arctic seas. In want of the skin of an adult or semi-adult whale, that of a young subject will be a very valuable acquisition. The possession of the various viscera, the genital organs, the organs of the senses, the characteristic parts of the skeleton, is equally desirable. If, as may easily happen by falling in with a whale-ship, the zoologists of the expedition can procure some parts already cut up and mangled of a very large whale, they may, even in such a case, be useful to science, by bringing away specimens of the great nerves and principal vessels, selected to suit their convenience. Finally, such is the imperfect state of our knowledge regarding all these gigantic inhabitants of the Polar Seas, that correct drawings and measurements will constitute a very important addition to the documents which science now possesses. Of these, the zoologists of the expedition will find a clear and faithful résumé in the recent work of our fellow-member M. Frederick Cuvier on the Cetacea.

3*d*, The sea-horse, almost as rare as the narwhal, and scarcely better known. Some authors have hinted at the existence of two species, depending on the form of the tusks being sometimes more compressed, at other times more approaching to a conical form. These are probably only two varieties, but their definite distinction would not be, on that account, a less service to mammalogy.*

* On this subject see vol. i. p. 131, &c. of Memoirs of the Wernerian Natural History Society.—Edit.

4*th*, Lastly, the seals of the north, of which it is desirable that a series could be obtained, on account of the numerous varieties of age and sex presented by the greater part of the species; whence the extreme difficulty of determining the latter. Here again, M. Nilsson can render most valuable service, his assiduous researches regarding the carnivorous amphibia having enabled him to acquire, with a precision seldom attained, an acquaintance with the different species which inhabit the coasts of the Scandinavian peninsula.

After these large species of mammifera, the zoological interest of which exceeds that of all others, we particularly recommend that attention be paid to the land bears of the north, by reason of the doubts which some authors have conceived respecting the specific unity of the races which Linnæus comprehended under the name of *Ursus Arctos;* the lynxes of the north; the bats, and particularly the *Plecotus cornutus* of Jutland; the various genera of insectivora and gnawers, more especially the Pteromys of Europe; the squirrels, and the compagnols, among which there can be little doubt new species will be found which it will be very interesting to compare with their analogues of the north of Russia, so admirably described by Pallas; the Lapland owl in its different ages; the most northern species of the passeres, and all the gallinaceæ; the serpents and batracians of the north, among which species have been noticed which should be peculiar to Scandinavia; finally, the fishes of the lakes and of the rivers which flow into the Icy Ocean and the White Sea. Among the lower classes of the animal kingdom, all of which will present to the expedition, in their most northern representatives, objects of very great interest, we may particularly indicate the lithodes and other crustacea of the Arctic Seas, the terrestrial and fresh-water mollusca, and the limited number of lepidoptera which adorn the almost nightless summer of Lapland. Lastly, it will be very useful to make observations on the minute phosphorescent animals in the northern seas which the expedition must traverse, with a view to complete those which have been so often made, and even in recent times, by the zoologists of the *Bonite*, in seas so different by their geographical situation and temperature.

§ III. We cannot too strongly urge the zoologists of the expedition to determine, as exactly as possible, the geographical distribution of the various species, whether of the European or the Arctic Fauna. By completing, in this important point of view, their own observations by information acquired from other zoologists, and also from the hunters of the country, they will bring together materials of the utmost value, for determining the limits where our European species cease to occur, and where the Arctic species begin to appear. This question, no doubt, is not susceptible of a simple and general solution,—the European Fauna not being suddenly displaced, to give way to an Arctic one. It is partial solutions only that can be sought; and so difficult is the subject, that even these we cannot except in a certain number of instances, notwithstanding the confidence we place in the zeal of the zoologists of the expedition, and the readiness with which M. Nilsson and his fellow countrymen will second their efforts. But the examples for which we shall be indebted to them, however few in number, cannot fail to be of real interest to science, if they are well selected: that is to say, if they relate to well-determined species, about which no uncertainty can arise. It appears to us, that one of the means of attaining this object is, to fix on species the knowledge of which is not confined to persons learned in zoology alone, but which, on the contrary, have attracted general attention by some circumstances in their organization or habits. By this means the number of persons who may be consulted with advantage, will become much more considerable. In regard to species of small size, and no way remarkable for their internal characters or manners, zoologists alone can give information; huntsmen, on the contrary (and the chase as every one knows is the principal occupation of a considerable portion of the inhabitants of Scandinavia), may be consulted safely and profitably, when the point is to determine, for example, how far the common fox, hare, or French *mustela*, advance northwards; or how far southwards the isatis, changeable hare, and the zibeline extend. That the first succeed immediately to the second,—that they are separated from each other by a greater or less interval,—or, finally, that they co-exist in some places, and so to speak, meet each other on

the limits of their geographical distribution ; the result, whatever it may be, of the researches we have here indicated, must be registered with care, and cannot fail to lead to consequences of very great interest.

Finally, we invite the zoologists of the expedition to search for the analogies which may exist between the Fauna of the elevated parts of the Scandinavian Alps, and that of the low regions, more removed towards the north, which traverse the same isothermal line. Such relations have already been noticed by various authors in different regions, among others by M. Latreille in relation to Sweden itself as compared with our Alps and Pyrenees ; and they are of so great interest, that we ought to endeavour, by similar observations in other places, to confirm them and generalize them more and more.

Additional Notes to the Zoological part of the Instructions given by the Academy.

Some of these notes are designed to complete, others to illustrate in various points, the zoological instructions which have just been read. I at first drew them up, not for the press, but the personal use of the zoologists of the expedition, to whom they were given in manuscript. While placing them here, at the request of M. Gaimard, I must premise, that they have not been submitted either to the commission nominated by the Academy, nor to the Academy itself. They therefore appear in this place solely on the personal responsibility of the reporter.

Note A.—*On the Varieties of Locality.*

Being obliged to confine myself to narrow limits in my report, I could not insist so much as I desired on varieties of locality, and on the importance of studying them profoundly at the present time. The following extract from the Introduction to the Zoological part of the great work on the Morea, will supply what I was unable to state formerly. The question, in fact, presents itself in exactly the same light when we compare Greece with France, and France with Scandinavia, insomuch that the passage about to be quoted, although written about the animals of Greece, is applicable in every respect to those of the southern and middle regions of the Scandinavian peninsula.

" Since natural history, enriched by the discoveries of numerous travellers, embraces at once in its immense field, the beings of all the countries of the globe, naturalists, for a long time accustomed to the inspection of the animals and plants of their own countries, have made distant regions the habitual subjects of their most profound study. New Hol-

land and Madagascar, two countries in which nature is beautiful and varied, as in our own climates, but so different, that one would suppose it animated by another sun; South America, which has been truly called a New World; Africa, the vast country of monsters, as it was termed by the ancients; finally, India, an inexhaustible region to the naturalist, where the creations of all the other parts of the globe seem represented, by some beings dispersed, and as it were, lost in the midst of a creation altogether peculiar; such are the parts of the globe whose natural riches, explored for many years, are now, and will no doubt continue to be, the principal objects of the efforts and researches of travellers.

"The knowledge of the animated beings, or, to confine myself to the subject of my more especial researches, the knowledge of the animals of these countries, is found to be fruitful in curious, singular, and unexpected results. The zoologist is delighted with the study of those beings, in which each organ, each function, each habit, is in contrast with the organs, functions and habits of the animals of our own Europe. The progress of his observation conducts him, at each step, over a new land, and, by disclosing to him a multitude of unknown facts, imparts to him that pleasure, so highly relished by an instructed mind, which arises from the discovery and contemplation of new objects; a pleasure to which even the most ignorant are not insensible, urged onwards unconsciously by one of the noblest instincts,—the innate desire of seeing and knowing.

"The comparative observation of the animals of different parts of Europe cannot present us with similar attractions. Must it follow, then, that the study of these must be without interest? Can they not lead us to some results of advantage to science? Are the facts most striking for their novelty, necessarily the most fertile in instruction? We think not; and perhaps the following considerations will free this opinion from the paradoxical character which it seems at first sight to present.

"Undoubtedly, if zoology had no other aim than the drawing up of a catalogue of the innumerable animals which replenish the surface of the globe—the further enrichment of a catalogue already immense; in other words, if the discovery of new species were the only kind of progress required by science, the comparison of beings belonging to regions separated by enormous distances, or by important differences of temperature or climate, would alone deserve to become the subject of our labours, since it alone could present us with organic diversities rendered evident by decided and remarkable characters. But does zoology deserve the name of science, if it continue purely descriptive and unphilosophical, and lead to no other results than such as flow from labours of that kind? And if it be of importance to it to enumerate animal species with exactness, and carefully to note the differences which distinguish them, are not the origin and formation of species, the nature and causes of their differences, likewise questions of real, nay, of immense interest? Now, it is evident that it is, above all other things, the comparison of beings in countries but little removed, and differing little from each other, that can throw light

on these questians, complicated and difficult, it is true, but still accessible to our reason and observation. There, differences of a less decided kind are established, under the action of real and active, but less powerful, causes of diversity; there, the influence of local circumstances is often shewn in a very evident manner, which, leaving all the essential conditions of the common ground-work untouched, still make it undergo a slight alteration. In other words, it is there that types differing slightly from each other are established, which all zoologists designate by the name of *local varieties*; thus unanimously recognising in application, and, if I make use of the expression, in practice, that powerful modification of exterior agents, which so great a number of them deny more or less completely in their theories. These *varieties of locality*, tending to establish the passage of one species into another, have hitherto been scarcely any thing else, in natural history, than a subject of serious difficulties and continual doubts; and such will continue to be the case as long as the distinction of beings, and the precise knowledge of differences, shall be the principal and almost the only objects which zoologists propose to themselves. Now, on the contrary, when the progress of science has reached such a point (and we may certainly say that we have almost reached it), that there will not only be a desire to note, but also to appreciate differences, not only to classify species, but to explain their formation, the study of *local varieties*, those *demi-species*, as they may be called, will become one of the favourite subjects of investigation with zoologists, since it will be one of the most fruitful in definite results, results which will have an immediate application to natural philosophy.

"Another kind of interest also attaches itself, in some particular cases, to the comparative study of beings belonging to regions not widely separated. Certain countries, placed between two zones very different in climate, and intermediate to each other by their physical conditions, such as situation, ought, according to the particular circumstances of their topographical disposition, to present us with a distinct passage between two orders of things, or, according to an expression already admitted into science, and previously employed in this article, between two different creations. These are truly what may be called, to borrow an expression from another branch of natural history which best conveys the idea, *transition countries*; and if the whole of their productions, deprived of a striking and well marked special character, but, on the contrary, sharing at the same time the conditions of neighbouring regions, do not call the attention by the appearance of its contrasts, to those natural oppositions which speak so loudly to the imagination, the study may at least reveal to the observer a multitude of those secret relations, concealed analogies, and fruitful inductions, which contemplative minds delight to discover under the thick veil which conceals them from the vulgar eye."

NOTE B.—*On the Beaver.*

The beaver of Europe has been considered by some authors as a different species from the American beaver; according to others, it is a mere variety of the *Castor fiber*, which is thus scattered over the cold and temperate parts of the two continents; according to others again, and M. Cuvier is among this number, the question is a doubtful one. The possession of the beavers of the north of Europe, in particular, of Scandinavia and Russia, and their comparison with those of France and Germany, and at the same time with those of America, can alone furnish the means of solving these doubts.

The young, even those of France and the United States, are still unknown to us. We are equally in want of precise information regarding the changes of the fur at different seasons. Lastly, I recommend to the attention of zoologists of the expedition the white beavers, which are said to be almost common in Lapland. Do they continue so at all seasons? Or, rather, do a greater or less number of individuals become white in winter?

But, of all the questions that attach to the natural history of the beaver, undoubtedly the most interesting is that referring to its manners, about which much remains to be learned, notwithstanding all that has been already collected on the subject. It is now known to every one, that the beaver does not construct a habitation in every country where it occurs; but we are not equally well acquainted with all the differences of its manners, according to the countries. In certain localities, for example, on the banks of the Rhone, Gardon, Danube, Rhine, Weser, Lena and Yenisei, it lives solitarily in burrows of a very simple construction, and its industry seems scarcely to surpass that of our water-rat. In other places, and even at times in the same place, particularly on the Lena and Yenisei, it assembles in family groups, or even associates in numerous tribes, then forming what is sometimes called a *community of beavers*. Its burrows are then more extensive, complicated, and more skilfully constructed. Lastly, in North America, united in *communities*, it forms not only burrows, but, as every one knows, little huts constructed with infinite art, often protected by immense mounds, to which no other gnawer (with the exception of the Ondatra) presents any thing comparable.

Does the beaver of Norway, and that of Lapland, live solitary, in families, or in communities? What degree of industry do they shew in the construction of their habitations? The incomplete information we now have seems to shew, that the beavers of the north live in many places in communities, and display much art in forming their burrows on the banks of rivers; but we know not if communities exist equally in Norway and Lapland; how many individuals they usually contain; in what places the solitary beavers, or those which assemble in families only, reside; what variations their burrows present in form, dimensions, and disposi-

tion; lastly, whether, in any locality, even in the north of Scandinavia, any thing can be observed to indicate at least a tendency to the construction of huts.

The solution of all these questions will be attended with great interest, for they are not only curious in themselves, but connected with many other questions, some of which are of high philosophical importance.

It has been long since asked why the American beavers are builders, and those of Europe burrowers? Two replies have been given, and both of them are found to be false. The attentive examination of facts has overturned what at first appeared plausible in both.

Thus a very simple, and to appearance, very satisfactory explanation, has been deduced from the specific difference of the American and European beavers. Each species has its distinct manners, as well as its proper organic characters. In this light, the genus Castor would only exhibit between the two species differences analogous, although more decided and remarkable, to those which we are accustomed to meet with between different species of the same genus. But, in reality, it is not so. Without the necessity of reminding the reader how doubtful the specific difference of the American and European is, facts exist which effectually destroy this explanation. The beavers of America are not all builders. Those of Louisiana confine themselves, like our own, to digging burrows, and, for the most part, do not live in communities. On the other hand, an extremely curious observation, made by my father in the menagery of the Museum of Natural History,* proves that our European beavers themselves become builders, when circumstances put them to the necessity of preparing a shelter; and that, when placed in a situation where they cannot dig, they dispose their materials, although very imperfectly, into a building. The hypothesis, then, of a fundamental diversity of instincts in correspondence with the specific difference of American and European beavers, falls to the ground before two facts, equally conclusive—the existence of burrowing beavers in America, and the demonstrated possibility that the European beavers would form, in certain cases, buildings of a more or less complicated kind.

Another hypothesis, much more generally admitted, is that which attributes the simplicity of the beavers' works in Europe to their being so closely in the neighbourhood of man, and the numerous causes of disturbance to them arising from that circumstance. According to this view, the beaver would naturally be a builder. He would in former times live everywhere in large communities, and in every place employ his industry in constructing houses similar to those now found in North America. The multiplication of man alone, the unceasing extension of his dominion over the surface of the globe, would alone have obliged the beaver to diminish his works more and more, and finally to take refuge in simple

* See Memoires du Museum, tome xii. page 232.

burrows, where he lives solitarily, or at most in families. This rather ingenious explanation may in part be true; but, unquestionably, it is not all the truth. It is not only in places frequented by man, but likewise in regions which he scarcely ever visits, that scattered and burrowing beavers occur. The virgin forests of Louisiana present us with examples, so much the more worthy of notice, from it being impossible to ascribe to any specific difference this remarkable discrepancy in the manners and habits of the beavers of Louisiana and those of Canada.

M. Desmoulins, to whom we are indebted for having already directed attention to the last-mentioned fact, mentions another, which it is of consequence to notice in this place. It is that the Ondatra, so well named by some of the people of North America, the *beaver's younger brother*, builds a habitation, like it, in cold regions, and like it also, becomes a burrower in temperate regions. Climate, then, seems here to exercise a marked influence, and if it does not act alone, it must at least be placed side by side with the influence produced by the vicinity of man.

It is in this point of view that exact notions respecting the beavers of Norway and Lapland, appear to me to be of such great interest to science. By using every effort to obtain them, the gentlemen of the expedition will not merely enrich the history of the most remarkable genus in the entire order of gnawers with new facts, but they will contribute to prepare the way for the solution of one of the highest questions in philosophical zoology, the influence of climate and of external circumstances in general on the manners of animals.

[Note C.—*On the Changes of Fur or Plumage according to the seasons.*

I have not here pointed out particularly to the attention of the zoologists of the commission, the species which become white or of a lighter colour in the winter. Many observations have already been made on them by travellers, and I have myself been able to follow the remarkable changes of colour in the Isatis which M. Gaimard sent from Iceland to the menagery of the Museum of Natural History. Here, however, as in every department of zoology, what remains to be done is much greater than what has been already accomplished.

I shall point out a few subjects for investigation on this head.

1*st*, To draw up an exact list of all the species which become perfectly white in winter, and of those which only assume (as appears to be the case with squirrels) lighter tints.

2*d*, To determine, by exact information, whether there be individuals which remain white all the year, and others, on the contrary, which preserve the deep-coloured fur or plumage during the winter.

3*d*, To investigate in what order, and in what manner, one colour is substituted for another; and, in particular, whether it be true that the change of colour takes place in many species without change of feathers or hair, a circumstance which would be contrary to my own observations on the Isatis, but which is admitted as certainly occurring in many nor-

thern countries, particularly in Terra-nova, according to our illustrious colleague M. Larrey.*

4th, To determine, and, if possible, preserve by good coloured drawings, all the modifications which take place in the colour of the skin, particularly its naked parts, in the eyes, beak, &c. at the time when the change in the colour of the feathers or hairs occurs; to examine if the interior membranes, for example, the mucous membranes of the mouth, do not change their colour at the same time as the external skin.

5th, Lastly, to determine whether any changes of colour, according to the seasons, more or less marked, occur in any other instances than in animals covered with feathers or hair.

NOTE D.—*On the Suspension of Life in Batracians from the effects of cold.*

However imperfect may be the experiments which I made on this subject, it may not be useless to give a short account of them in this place; they will at least furnish some useful hints to the zoologists of the expedition, if they find themselves placed, as I hope they shall, in favourable circumstances for the successful study of a subject upon which I was the first to enter.

These experiments were made in the winter of 1828–29 on three species, the common frog, common toad, and the calamite toad. They did not succeed on the former of these species, for reasons which it is unnecessary to mention here, but of which it is enough to say, that they are no way opposed to the success of the experiments which may be attempted hereafter. In the case of the toads, including both species, I established the facts which may be thus shortly stated.

Toads may be congealed, without losing life, so far that the spaces between the muscles become filled with small pieces of ice, and all, or nearly all, the functions suspended. Their restoration, however, is possible, provided the rising of the temperature be properly graduated; they may even be completely restored and recover all their wonted agility in eight or ten minutes.

In their frozen state, the toads on which I experimented gave no signs of life; their body was hard, rigid, and absolutely like a piece of a frozen carcass. It was impossible to make their limbs perform the least movement; the smallest effort would break them; the soft parts were so hardened as to be as brittle as bone, and when broken not a drop of blood flowed.

The toads on which I made these observations, were placed by me, in the beginning of winter, in a box filled to a certain height with earth. The calamites almost immediately buried themselves in the earth, the common toads did so only when the cold became intense. It was in the holes thus dug by themselves that the freezing of them was effected.

* See his Memoires de Chirurgie Militaire et Campagnes, tom. i. pp. 31 and 32.

Their return to life, truly surprizing by its rapidity, was brought about, in my experiments, by immersion in water slightly heated. Almost at the moment when the particles of ice were melted, the limbs and skin recovered their flexibility, and I saw the animals begin to move; at the same instant their eyes, hitherto very much sunk, suddenly became prominent, and in a short time, about ten or even eight minutes, they seemed perfectly restored.

The toads which I caused to freeze too rapidly, whether in water or air, never came again to life; at the most they made a few faint movements, soon followed by death.*

Instructions in Zoology and Animal Physiology, for the British Antarctic Expedition. †

1. *Marine Invertebrata.*—The animals which it is desirable to preserve, and which may at first present themselves to the notice of the naturalist in the present expedition, are the floating marine Mollusca and Crustacea, and those which inhabit the Sargazzo or Gulf-weed.

With respect to the Mollusca, all the species of the Cephalopoda or Cuttle-fish tribe, and all the Pteropoda or lower organized floating Mollusca, should be preserved. If taken alive they should be allowed to die gradually in sea-water, by which means they commonly remain in a relaxed state, and display more of their natural outward form. When dead they should be soaked for a short time in fresh water, and then put into spirit; or, if transparent, in the saline solution,‡ to prevent decomposition, which otherwise rapidly takes place. To each specimen should be attached a number, stamped on sheet tin, corresponding to the entry-number in the Catalogue, in which should be noticed the kind of locomotion, or other vital phenomena, and the colour of the living animal, the latter being speedily altered or lost in the preserving liquor. The larger Crustacea will be liable to become putrid in spirit,

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* Voyage en Islande et au Groenland, par M. P. Gaimard, t. i. pp. 349, &c.

† Prepared by the President and Council of the Royal Society.

‡ Common salt, . . 1 part.
Alum, . . . 2 parts.
Boiling water, . . 10 parts.
Filter the solution when cold.

unless the soft mass, which fills a large portion of the body, consisting of the liver, &c. be removed. Each specimen of this class, excepting the very minute ones, which will be best preserved in small phials or glass tubes, should be wrapt in a piece of very soft, thin linen or cotton cloth, to prevent the legs from being intermixed or lost, as they are very likely to fall off after having been a short time in spirit.

A very important object of investigation is the development of the Crustacea, from the earliest period at which they can be observed to the perfect state. They may be readily examined before they leave the egg, by opening the egg under a single microscope. Drawings of these changes are very desirable, and when practicable the eggs and young ones in different stages should be preserved in spirit in short glass tubes. The smaller oceanic Crustacea offer a prolific and hitherto unexplored field of investigation.

Among the floating Mollusca likely to be met with in the tropical latitudes is the Spirula, a small Cephalopod with a chambered shell. An entire specimen of this rare mollusk is a great desideratum; and if it should be captured alive, its movements should be watched in a vessel of sea-water, with reference more especially to the power of rising and sinking at will, and the position of the shell during those actions. The chambered part of the shell should be opened under water, in order to determine if it contain a gas; the nature of this gas should likewise, if possible, be ascertained. As a part of the shell of the Spirula projects externally at the posterior part of the animal, this part should be laid open in the living Spirula, in order to ascertain how far such mutilation would affect its power of rising or sinking in the water. In the event of a living pearly Nautilus (Nautilus Pompilius) being captured, the same observations and experiments should be made on that species, in which they would be attended with more precision and facility, as the species is much larger than the Spirula, and its shell external. The towing net should be kept overboard at all practicable periods, and drawn up and examined at stated intervals, as some of the rarest marine animals have been taken by thus sweeping the surface of the sea.

A sketch or drawing of molluscous and radiate animals, of which the form and colour are liable to be materially altered by death, or when put in spirit, will aid materially in rendering the description of the species useful and intelligible. The Echinodermata should be soaked in fresh water previously to their being put into spirit. Care must be taken not to crowd too many soft-bodied invertebrata in the same bottle, and to change the spirit or preserving liquor at least once, if not oftener.

2. *Fishes.*—The mode and speed of swimming, living colour, temperature, and any other peculiarity, should be noticed before placing the specimen in spirit.

In very large specimens of the Shark or Ray kind, a section of the jaws, with a part of the vertebral column, should always be preserved as wet preparations, and the remainder of the jaws and vertebral column in a dry state. The eyes, eyelids, and part of the surrounding skin, should be preserved in the saline solution. In less bulky specimens, the entire head should be taken off by dividing the fish below the heart across the upper part of the liver, by which means the mouths of the oviducts, if it be a female, the heart, gills, and head are all preserved together.

The tail of a shark may be taken off a little below the anus, and the trunk alone preserved for examination. If the trunk be too large, it should be cut through above the pelvis, and the parts contained in the hinder portion, as the claspers of the male, should be preserved in spirit. If the specimen be a female, separate the two oviducts through their whole length, where they run along the abdomen, on each side of the spine, but keep them attached to the cloaca and its surrounding parts.

If with young or eggs, take the whole out in the same way without opening the oviducts.

The heads of all fishes should be preserved, when specimens are too large to be preserved entire.

All external parasites, and those which infest the gills of fishes, should be preserved. The alimentary canal should, in all cases, be examined for the presence of entozoa, which, if

adherent to the coats of the intestine, should be preserved with the part to which they are attached. One of the most interesting fishes of the Southern Seas is the Port Jackson Shark (Cestracion Philippi). Moderate-sized specimens of this species should be preserved entire; and the head, vertebræ, with the dorsal spines, viscera, and especially the impregnated oviduct, should be preserved. The Southern Chimæra (Callorhynchus antarcticus) merits also the special attention of the naturalist, and the same specimens of this species should be preserved as of the Cestracion.

3. *Reptiles.*—Specimens of turtle should be carefully examined for parasitic animals; a curious Barnacle (Chelonoiba) and a Leech (Hirudo branchiata) are occasionally found adhering to these marine Reptilia. In the event of the expedition touching at the Galapagos Islands, specimens of Amblyrhynchus, a lizard of marine habits, should be secured, and the particular locality of the capture noted.

4. *Birds.* — The Chionis or Sheath-bill of the Falkland Islands and Cape Horn.

The Great Penguin (Aptenodytes).

The Penguin of the Isle San Lorenzo.

Of these rare and desirable birds, besides the prepared skins, the entire body should be preserved in spirits for anatomical purposes. The young of the Great Penguin, and the eggs at different stages of incubation, should likewise be similarly preserved.

5. *Mammalia.*—The skulls, skeletons, and viscera of a specimen of each species of the Cetacea of the Southern Ocean are worthy of being preserved. With respect to the Sperm Whale, an entire fœtus, or, if of large size, the brain, eyes, pharynx, larynx, and blowholes, and the viscera; a part of the impregnated uterus; the ovaria and a portion of the membrane of the fœtus; are all parts worthy of preservation. The same observations apply to the great Elephant Seals (Phoca Cystophora) (proboscidea); of which the skull and skeleton of both male and female are very desirable.

The skulls or skeletons of all the species of the Southern Seals should be preserved, the sex being noted.

6. *In particular Regions.*—In Australia or Van Diemen's Land the following species are more especially worthy of attention.

Thylacinus Harrisii, Hyæna of the colonist.

Of this species the skeletons of the male and female, detached skulls, an entire specimen in the saline solution for dissection, the viscera, and more especially the impregnated uterus, and a young specimen for the changes in dentition, are particularly desirable; such specimens not having been as yet transmitted to the museums of this country or on the Continent.

The skeletons, skulls, and female organs of every marsupial quadruped, and of the Ornithorhynchus and Echidna (or Porcupine of the colonist) should be preserved.

The smaller Mammalia of Australia, whether marsupial or rodent, should be preserved in spirit, and particular notice taken of their locality and habits.

Among the Birds of Australia the Lyre Pheasant (Menura) would be an interesting subject for anatomical investigation. Of this species are wanting the skeletons of a male and female, and of the young bird; and the entire body of both sexes in spirit, or the saline solution. The same with respect to the large-billed Cuckoo (Scythrops), and Sea-Partridge (Glareola).

In New Zealand, similar preparations should be obtained of the Megapodius, and of the Apteryx Australis.

With regard to birds, it may be observed, that if spirit be injected down the windpipe, it will pass through almost the whole body by means of the air-cells. In the case of a quadruped preserved in spirit, or in the saline solution, it is proper to inject the preserving liquor into the abdominal cavity and intestinal canal.

Meteorological Observations made at the Imperial Academy of Sciences of St Petersburg, from 1822 *to* 1835, *and calculated by* M. A. T. Kupffer.*

The meteorological observations which M. Kupffer has just calculated and published, were made by M. Wisniewsky. They are not the first which have been made at St Petersburg. Some go so far back as 1726; but they were printed with few details in the *Memoirs of the Academy.* Nevertheless, M. Kupffer, after having collected them into several tables, announces that he will compare them with those recently executed, in order to deduce from this comparison certain conclusions concerning the changes which, it is said, have for some time been observed in the climate of St Petersburg.

From the numerous results which the volume just published by M. Kupffer contains, we have selected some, which appeared to us of great interest, whether considered in their own nature, or on account of the geographical position of the place where they were obtained. But before placing them before our readers, we shall mention some preliminary observations of M. Kupffer concerning meteorology, as remarkable for their justness, as they are instructive for the summary which they present of the labours undertaken at St Petersburg on this subject.

"Meteorology is the natural philosophy of the people of the world; it observes nature in general, and such as she presents herself of her own accord, and requires no great extent of acquirements, either in the knowledge or management of instruments; it employs no skilfully combined artifice to surprise the secret of nature. It is for this reason also, that it is one of the most anciently cultivated sciences. We have a work of Aristotle on Meteorology; and in Egypt, the study of the succession of the seasons, and the rise of the Nile, a true meteorological phenomenon, ascends to the highest antiquity; the origin of meteorology and that of astronomy are mixed up together in all the monumental and traditional history of early states.

* Biblioth. Univ. de Geneve, No. 41. (May), published 29th June 1839, pp. 176, &c.

" Meteorology, sharing with astronomy the advantage of a high antiquity, has not, in like manner, shared its fate. Astronomy presents to us a concatenation of periodical phenomena, and every century added to the observations, adds at the same time to the accuracy of the results; but meteorology, which embraces so many disjoined phenomena, in which no secular period has yet been observed, gains nothing by ascending to the times of its origin; on the contrary, it only encounters, in the history of its infancy, superstitions, and a profound ignorance of the laws of nature. In early times, the imagination of man, separating all physical explanation from observed phenomena, and misunderstanding entirely the natural relation of causes and effects, supposed a free will, a Deity, wherever immutable laws were afterwards discovered. At a subsequent epoch, the study of meteorological phenomena, abandoned to sailors and shepherds, only produced gloomy superstitions, conformable to the spirit of the age which made the genius of evil preside over all worldly things. And at present, how many false and unfounded ideas are those which are believed even in the most enlightened class of society! how absurdly people often talk about good and bad weather, about the influence of the moon in meteorological phenomena, and about the predictions of the barometer!

" This long infancy of meteorology,—this slow development of a science which occupies so many, and the study of which supposes so few means, is owing to many causes. The origin of meteorology goes back, as we have seen, to a remote epoch, when men did not yet possess the means necessary to resolve, by exact observations, such complicated and difficult questions; they pronounced the name of meteorology as a distinct science, without being able to constitute it as such. In ancient times, they did not know the art of making experiments, that is, of producing on a small scale, and of approximating, so to speak, by the observer, the great phenomena of nature,—of isolating them,—of stripping them of all that is accidental, and which complicates the question, without belonging essentially to it. It is only in the eighteenth century that men have begun to discover, by multiplied and skilfully combined experiments, certain laws,—certain agents,

which perform an important part in meteorological phenomena; and ever since, these new discoveries have been made subservient to explain these phenomena. What progress, for example, could the theory of the electricity of the clouds have made, if we had been always obliged to observe these phenomena on the great scale, such as nature presents them to us, striking the observer with amazement rather than with curiosity, crushing, so to speak, his attention, by the impetuosity of their shock? It was by rubbing, at first, a small bit of amber, and afterwards a plate of glass, against a piece of cloth; it was by causing the electricity developed by this friction, to act on light pith balls, that we discovered the identity of the electricity of our machines with the matter which charges the stormy clouds, and which darts from them in flashes to destroy all it encounters; and ever since, all the circumstances which accompany these terrible phenomena have been studied without danger, by exhibiting them on a small scale in our laboratories.

" But there is still another cause which has retarded the developement of meteorology, while astronomy, starting from the same point, has followed a more secure and rapid route. Astronomy is occupied with phenomena, which present, at first sight, great regularity and remarkable simplicity; greater complication only manifests itself after a long course of observations, and the employment of more perfect instruments. It is thus that astronomy has passed from the simple to the complicated, and that the human mind, allured by the hope of easy discoveries, has engaged with ardour in a path, the difficulties of which only shewed themselves in proportion as the investigation advanced, and whose obstacles, although continually increasing, always kept the same relation to the strength acquired. In meteorology, precisely the contrary has taken place; meteorological phenomena present, at the first sight, extreme complication and irregularity; and it is only by the force of observation, and the employment of exact instruments, that we have at last arrived, after many fruitless attempts, at the discovery of certain general laws.

" The neglect into which meteorology had for a long time fallen, recommends it particularly to academies and learned

societies. The isolated philosopher will always choose as the object of his labours researches whose result is not too remote. He wishes to finish himself what he has commenced, and to gather the glory of the discoveries which his labours have prepared. Now, the results which meteorological observations can furnish to science are only to be obtained after much delay; and farther, these results are not numerous. Years of careful observations, observations which demand the attention and assiduity of every day, only offer as results certain means, which, referring merely to a single point of the earth's surface, are lost in the number of results obtained elsewhere. Such assiduity in labour, such self-denial, demand greater efforts than an isolated life can offer; the division of labour alone can guarantee the success of such an enterprise. It is only the active emulation which is established so quickly by the community of labour, and the deep conviction that nothing one does shall be lost, that can support the interest of so painful an occupation. Meteorology does not possess, like astronomy, establishments specially destined to extend its domain, and to furnish, without interruption, regular observations: this science has not yet, notwithstanding its importance, attracted the attention of Government to such a degree as to become an object of its particular care, and to call forth considerable sacrifices in its behalf. Poor and forsaken, it flees entirely for refuge to the fostering care of learned societies.

"The Academy of Sciences of St Petersburg is one of those where meteorology has been cultivated for many years with the greatest care. The meteorological observations of St Petersburg reach back to the year 1726. The first which were made in this capital are not, indeed, so complete and exact as those of the present day; at that epoch, meteorology was yet in its infancy. The register of the observations since the year 1726 has preserved the maxima and minima of the barometrical heights of each month, the number of days of rain and snow, and the dates of floods and auroræ boreales which happened each year. Since 1744, the minima and maxima of the temperature of each month are noted, expressed in degrees of Delisle's thermometer. Since 1769, the barometrical height of every day is marked, and the means of these observations

for each month are given. Since 1781, the thermometer has been observed morning and afternoon of each day, at what precise hour is not said: it appears that the hours of observation were not always the same.

"Since 1803, the meteorological observations of St Petersburg present more regularity. Observations are made three times a-day, morning, afternoon, and evening; the direction of the winds each day is noted. But it is especially since 1822 that we possess good meteorological observations; since that period, their execution has been confided to the academician M. Wisniewsky. The thermometer, the barometer, the direction of the winds, and the state of the weather, are regularly observed thrice a-day, at 7 o'clock in the morning, 2 in the afternoon, and 9 in the evening. The quantity of rain and snow is also marked. The acknowledged ability of the observer guarantees the accuracy of the observations: no precaution has been omitted, no interruption has taken place. It is undoubtedly a matter of regret, that other occupations have not allowed M. Wisniewsky to observe more frequently; three observations a-day, executed at hours which do not coincide with those of the minima and maxima, are not sufficient to determine with exactness the horary variations of the barometer, an object which, for some time, has fixed the attention of natural philosophers, and with which so many distinguished observers are almost exclusively occupied. To know exactly the daily gradation of temperature, a greater number of observers would be necessary; it would be necessary at least to observe the minimum of each day. No hygrometrical observations have been made; the hygrometer of Saussure, the most exact of those that were known when the observations began, and, consequently, the only one which could well be employed, is too uncertain in its indications, and too easily deranged. Nevertheless, notwithstanding these defects, the observations of M. Wisniewsky present so complete a summary, and offer so exact a table of the climate of St Petersburg, that I believe I shall render an important service to science by publishing them in all their details, after I have calculated them according to the rules generally admitted. I publish them in all their details,

because I think that, hereafter, an attentive examination of these observations, taken separately, or compared with observations executed elsewhere, will discover many interesting relations, connections not yet remarked among the values which they offer. Meteorology, in order to establish its reasonings on a solid basis, has hitherto been almost exclusively occupied with *means;* it has not yet entered into details of the daily values which compose them: but a time will come, and perhaps is not far distant, when the meteorological phenomena of every day, their succession, their mutual dependence, their coincidence in many proximate points, their limited extent, in a word, those daily expressions of climate which exercise so great an influence on the well-being of society, will excite a higher influence than the means of temperature, of barometrical height, and of humidity, the discussion of which will be reserved for more abstract study.

"The meteorological observations of M. Wisniewsky comprise 13 complete years from 1822 to 1834. The days are reckoned according to the new style, although it is not yet adopted in Russia. The calculations I have undertaken, or caused to be undertaken under my eyes, refer principally to the barometrical and thermometrical means, to the influence of the winds on these means, and to the influence of the moon on the height of the barometer. The results of these calculations precede the observations themselves, a collection of which will be found at the end of this memoir."

We shall now proceed to detail some of the results. We shall begin with the barometrical heights, which have all been reduced to the temperature of 63°.5 Fahr.*

The mean barometrical pressure deduced from 13 years' observation is 29.9 inches English.† The mean of the *maxima* heights of each month has been 30.5 inches; the mean of the *minima* heights 29.26 inches. The greatest barometrical height during the 13 consecutive years has been 31.29; it

* It is right to mention, that the thermometric and barometric numbers given in this paper have all been reduced to the English scale.—Ed.

† This same height, the temperature of the mercury being 32°, is 29.887.

happened the 3d of January 1826. The smallest height has been 28.35 ; it took place the 6th of February 1825. A particular table has been calculated to ascertain the influence which the moon exercises on the atmosphere ; it contains the means of the barometrical heights observed during the syzygies and the quadratures, that is, the day itself of the syzygy or the quadrature, the two days which precede, and the two which follow it.

SYZYGIES.

7 h. a.m.	2 h. p.m.	9 h. p.m.
29.9536	29.9586	29.9646

QUADRATURES.

7 h. a.m.	2 h. p.m.	9 h. p.m.
29.953	29.9454	29.9588

The difference between the barometrical height of the syzygies and that of the quadratures is so small, that one may, at St Petersburg as at Paris, regard the influence of the moon on the atmospheric tides as nothing.

Another table which we also insert here, sums up all the observations relative to the influence of each kind of wind on the barometrical height of all the years from 1822 to 1836. This height has been calculated separately for each wind.

	Means.
For the N. wind, ...	29.917
... N.E.	30.006
... E.	30.007
... S.E.	30.016
... S.	29.925
... S.W.	29.924
... W.	29.908
... N.W.	29.833
Calm, ...	30.048
Mean, . .	29.954

From this table it appears that, at St Petersburg, the direction of the winds has no influence on the mean barometrical height, and that their effects are completely counterbalanced in the course of the year. Of all the winds, the south-east causes the greatest, the north-west the least, barometrical height ; but the absolute maximum takes place during

calms. We see, then, that all the winds, from whatever quarter they blow, depress the barometer, but this depression is compensated by the influence of the calms.

We shall now say a few words with regard to the observations of temperature. The mean temperature of St Petersburg, as deduced from all the observations made at 7 h. A. M., 2 P. M., and 9 P. M., is 39°.61 F. The mean of the lowest and of the highest temperatures observed in each month, at the hours indicated, is 39°.51; that is, obviously the same. The most elevated temperature during the 13 years was 87°.8; it happened the 24th of June 1827; the lowest temperature was—22°.67; it happened the 7th of February 1823. Hence it appears, that, between the highest and the lowest temperature, there is a difference of 110°.47.

In a memoir read before the assembly of the naturalists of Germany, met at Jena, M. Madler attempted to demonstrate that the phases of the moon exercise a remarkable influence on the temperature of the air at the earth's surface. M. Kupffer wished to see if the observations of St Petersburg would indicate a similar influence. For this purpose, he calculated separately the mean temperatures of the syzygies and quadratures for 14 consecutive years; that is, from 1822 to 1835 inclusively. He took, as is generally done, the day of the phasis itself, the two days which precede, and the two which follow it. The means are:

New Moon.	Full Moon.	First Quarter.	Last Quarter.
39°.65	39°.24	39°.89	39°.65

In the course of the 14 years referred to, there were 173 new moons, and as many full moons and first quarters, and 174 last quarters. Each of the first three means, therefore, stated above, is the result of 865, and the last of 870 observations. If we might be permitted to draw any conclusion from so small a number of observations, the full moon would have the power of lowering the temperature. We must, however, remember, that the means themselves do not present a regular gradation; the influence of the first quarter ought to be the same as that of the last; the last, on the contrary, is as great as that of the new moon.

The mean quantity of rain and snow fallen from 1822 to 1834 inclusively, is 16.36 English inches, of which 11.81 are rain, and 4.54 snow, but measured after being melted.

M. Kupffer adds, in terminating this labour, of which we have only presented a very abridged extract, that, since the year 1835, meteorological observations have not been made at the Academy of Sciences, but at the Institute of Mines, on another plan, and by other observers, and that they are annually printed in a separate publication, the first number of which has already appeared under the title of,—*Meteorological and Magnetical Observations, made throughout the Russian Empire, arranged and published by A. T. Kupffer.*

On the Boracic Acid Lagoons of Tuscany. By JOHN BOWRING, LL.D.*

THE borax lagoons of Tuscany are entitled to a detailed description. They are unique in Europe, if not in the world; and their produce is become an article of equal importance to Great Britain as an import, and to Tuscany as an export. They are spread over a surface of about thirty miles, and exhibit from the distance columns of vapour, more or less according to the season of the year and state of the weather, which rise in large volumes among the recesses of the mountains.

As you approach the lagoons, the earth seems to pour out boiling water as if from volcanoes of various sizes, in a variety of soil, but principally of chalk and sand. The heat in the immediate adjacency is intolerable, and you are drenched by the vapour, which impregnates the atmosphere with a strong and somewhat sulphurous smell. The whole scene is one of terrible violence and confusion—the noisy outbreak of the boiling element—the rugged and agitated surface—the volumes of vapour—the impregnated atmosphere—the rush of waters—among bleak and solitary mountains.

The ground, which burns and shakes beneath your feet, is

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* From Dr Bowring's Report on the Statistics of Tuscany.

XUM

covered with beautiful crystallizations of sulphur and other minerals. Its character beneath the surface at Mont Cerbole is that of a black marl streaked with chalk, giving it, at a short distance, the appearance of variegated marble.

Formerly, the place was regarded by the peasants as the entrance of hell, a superstition derived no doubt from very ancient times, for the principal of the lagoons and the neighbouring volcano still bear the name of Monte Cerboli (*Mons Cerberi*). The peasantry never passed by the spot without terror, counting their beads, and praying for the protection of the Virgin.

The borax lagoons have been brought into their present profitable action within a very few years. Scattered over an extensive district, they are become the property of an active individual, M. Larderel, to whom they are a source of wealth, more valuable perhaps, and certainly less capricious, than any mine of silver that Mexico or Peru possesses. The process of manufacture is simple, and is effected by those instruments which the localities themselves present. The soffioni, or vapours, break forth violently in different parts of the mountain recesses. They only produce boracic acid when they burst with a fierce explosion. In these spots artificial lagoons are formed by the introduction of the mountain streams. The hot vapour keeps the water perpetually in boiling ebullition; and after it has received its impregnation during twenty-four hours at the most elevated lagoon, the contents are allowed to descend to the second lagoon, where a second impregnation takes place, and then to the third, and so forth, till it reaches the lowest receptacle; and having thus passed through from six to eight lagoons, it has gathered one-half per cent. of the boracic acid. It is then transferred to the reservoirs, from whence, after a few hours' rest, it is conveyed to the evaporating pans, where the hot vapour concentrates the strength of the acid by passing under shallow leaden vessels from the boiling fountains above, which is quite at a heat of 80° of Reaümur*, and is discharged at a heat of 60°†. There are from ten to twenty pans, in each of which the concentration becomes greater at

* The boiling point.

† 167° of Fahrenheit.

every descent till it passes to the crystallizing vessels, from whence it is carried to the drying rooms, where, after two or three hours, it becomes ready to be packed for exportation.

The number of establishments is nine.* The whole amoun t produced varies from 7000 to 8000 pounds (of 12 ounces) per day. The produce does not appear susceptible of much extension, as the whole of the water is turned to account; the atmosphere has, however, some influence on the result. In bright and clear weather, whether in winter or summer, the vapours are less dense, but the depositions of boracic acid in the lagoons are greater. Increased vapours indicate unfavourable change of weather, and the lagoons are infallible barometers to the neighbourhood, even at a great distance, serving to regulate the proceedings of the peasantry in their agricultural pursuits.

It had long been supposed that the boracic acid was not to be found in the vapours of the lagoons; and when it is seen how small the proportion of acid must originally be, it will not be wondered at that its presence should have escaped attention. In the lowest of the lagoons, after five, six, and in some cases a greater number of impregnations, the quantity of boracic acid given out does not exceed one-half per cent.; thus, if the produce be estimated at 7500 pounds per day, the quantity of saturated water daily discharged is a million and a half of Tuscan pounds, or five hundred tons English.

The lagoons are ordinarily excavated by the mountaineers of Lombardy, who emigrate into Tuscany during the winter season when their native Appenines are covered with snow. They gain about one Tuscan lira per day. But the works are conducted, when in operation, by natives, all of whom are married, and who occupy houses attached to the evaporating-pans. They wear a common uniform, and their health is generally good.

A great improvement in the cultivation, and a great increase in the value, of the neighbouring soil has naturally followed the introduction of the manufacture of the boracic acid. A rise of wages has accompanied the new demand for labour;

* The principal are Monte Cerboli, Monte Rotondo, Susso, Serazzano, and Castelnuovo.

much land has been brought into cultivation by new directions given to the streams of smaller rivers. Before the boracic lakes were turned to profitable account, their fetid smell—their frightful appearance, agitating the earth around them by the ceaseless explosions of boiling water, and not less the terrors with which superstition invested them,* made the lagoons themselves to be regarded as public nuisances, and gave to the surrounding country a character which alienated all attempts at improvement.

Nor were the lagoons without real and positive dangers, for the loss of life was certain where man or beast had the misfortune to fall into any of those boiling baths. Cases frequently occurred in which cattle perished; and one chemist, of considerable eminence, met with a horrible death by being precipitated into one of the lagoons. Legs were not unfrequently lost by a false step into the smaller pits (*putizze*), where, before the foot could be withdrawn, the flesh would be separated from the bone.

That these lagoons, now a source of immense revenue, should have remained for ages unproductive; that they should have been so frequently visited by scientific men, to none of whom (for ages at least) did the thought occur, that they contained in them mines of wealth, is a curious phenomenon; nor is it less remarkable, that it was left for a man, whose name and occupation are wholly disassociated from science, to convert these fugitive vapours into substantial wealth.

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* So unwilling were the peasants to settle in these districts, that very extraordinary encouragements were held out to them. In the commune of Monte Cerboli any inhabitant of the town may sow and reap whatever he pleases without requiring the consent of the owner of the soil; so it frequently happens that small tracts are cultivated which are particularly favoured by water or other advantages, and all the surrounding land left untouched. As the inhabitants have the primary right, the landlord generally abandons his property to the chance cultivation of the peasant, who leaves fallow nine-tenths of the land. In the district of Riparbella the landlords and cultivators have come to a sensible agreement, by apportioning the lands in equal moieties.

Many mineral waters are in the neighbourhood of the lagoons, some of which possess medical virtues, and are visited by the Tuscans in the bathing season.

Though to the present proprietor (the Chevalier Larderel*) the merit attaches of having given to the boracic lagoons the great importance they now possess, a succession of adventurers had made many experiments, and had produced a considerable quantity of boracic acid, but at a cost (from the expenditure of combustible) which left little profit.† The small value that was attached to them may be seen in the fact, that the largest and most productive district of the lagoons, that of Monte Cerboli, was offered in perpetuity, so lately as 1818, at an annual ground-rent of L.6 : 13 : 4 per annum, though it now produces several thousand pounds Sterling. The immense increase in their value arose from the simplest of improvements, the abandonment of the use of charcoal, and the application of the heat of the lagoons or soffioni to the evaporation of their own waters. Improvements, however, and very important ones, particularly by subjecting the waters to a succession of impregnations, had been gradually introduced by a Signor Ciaschi, and the importation of boracic acid from Tuscany into France, before 1817, had been between 7000 and 8000 pounds, of a quality gradually increasing in purity ; but Ciaschi perished miserably, in consequence of falling into one of the lagoons which he himself had excavated, leaving his family in a state of extreme poverty. His death (which happened in 1816) naturally threw a damp upon adventure. The experiments were resumed in the following year, and in the midst of violent claims and controversies, M. Larderel has become the monopolist of the boracic productions of Tuscany.

With the increased production of boracic acid has arisen an increased demand, growing out of the more extensive application of it to manufacturing purposes. In about four years, the quantity has been quadrupled by superior modes of ex-

* While these sheets have been passing through the press the Grand Duke of Tuscany has conferred on M. Larderel the title of Count de Pomerance.

† Hoefer first announced the presence of boracic acid in the Maremman districts, and Mascagni in his Commentaries suggests the manufacture of borax as an object worthy of attention. Professor Gazzeri in 1807, made experiments, which, however, seemed to shew that the quantity of boracic acid contained in the waters was too small to promise much success.

traction, and by greater care employed in the collection of the boracic vapour. In 1833, about 650,000 Tuscan pounds were obtained; in 1836, two millions and a half.

But it appears to me that the powers and riches of these extraordinary districts remain yet to be fully developed. They exhibit a great number of mighty steam-engines, furnished by nature at no cost, and applicable to the production of an infinite variety of objects. In the progress of time, this vast machinery of heat and force will probably become the moving central point of extensive manufacturing establishments. The steam, which has been so ingeniously applied to the concentration and evaporation of the boracic acid, will probably hereafter, instead of wasting itself in the air, be employed to move huge engines, which will be directed to the infinite variety of production which engages the attention of labouring and intelligent artisans; and thus, in the course of time, there can be little doubt, that these lagoons, which were fled from as objects of danger and terror by uninstructed man, will gather round them a large and intelligent population, and become sources of prosperity to innumerable individuals through countless generations.

On the Supposed Existence of Fluoric Acid, as an Ingredient in certain Animal Matters. By G. O. Rees, M. D., F. G. S. &c.*

In the year 1802, Morichini published a paper, in which he declared fluoride of calcium to be an ingredient in human teeth. He was led to examine that substance from having succeeded in detecting the fluoride in a specimen of fossil ivory. Mons. Gay Lussac repeated these experiments; and in the 55th volume of the "Annales de Chimie," he states, that the fluoride exists in recent as well as in fossil ivory; and that he had also succeeded in detecting it in the tusks of the wild boar. Fourcroy and Vauquelin subsequently published a memoir in the 57th volume of the "Annales de Chimie," in which they positively denied the existence of fluoride of

* From Guy's Hospital Reports, No. ix. October 1839, pp. 381, &c.

calcium as an ingredient either in recent ivory or the enamel of teeth; they found it, however, in the fossil ivory of Argenteuil and Lourque, though it did not appear to exist in specimens from Siberia and Lay.

Baron Berzelius has published a paper in the 61st volume of the "Annales de Chimie," in which he states, fluoric acid may be detected in human teeth, bones, urine, and may be demonstrated, in the latter case, by operating on the precipitate obtained from the excretion by means of lime-water. Since the publication of this paper by Baron Berzelius, the existence of fluoric acid, as a constituent of the animal substances above mentioned, has been acknowledged by chemists generally; and it is mentioned as such in the standard chemical works of the present day.

Having lately been engaged in the analysis of human bone, with more especial reference to those ingredients which have been stated to exist in small proportion, I was led to search particularly for fluoride of calcium. My experiments were made in the usual manner, by trying to obtain the corroding action of fluoric acid on a plate of glass, which was used as a loose cover to a platinum crucible, which contained the substance for examination, mixed with strong sulphuric acid. A gentle heat was applied to the bottom of the containing vessel. In this way, several specimens of human bone (both before and after calcination) were subjected to experiment; but in no instance could I obtain any action upon the glass.

The experiment which the Baron recommends, in order to obtain corrosion from bone earth, is, to distil equal parts of strong sulphuric acid and water upon it, until the measure of water is brought over. He states, that the distilled liquor, if evaporated in the glass receiver, will produce a corrosion. I repeated this experiment, using 100 grains of bone-ash, and an ounce of the acid mixture; but could obtain no action on the receiver, by evaporating the distilled liquor; nor was there any corrosion or opacity produced on any part of the apparatus.

During the evaporation of the last portions of the liquor, dense white fumes appeared; and there was some difficulty in vaporizing the whole of it. On neutralizing a portion with ammonia, and testing it with nitrate of silver, a yellow preci-

pitate of phosphate of silver was thrown down. A further examination shewed the presence of sulphuric acid, and traces of hydrochloric acid. I was much surprised to find phosphoric acid in this result of aqueous distillation, as the heat had not been urged during the process; for I had considered that acid was of too fixed a nature to volatilize with water at so low a temperature. It appeared to me now, that the presence of phosphoric acid, in this distilled liquor, might be a source of fallacy in the above experiment for establishing the presence of fluoric acid as a constituent of human bone; for it is a well known fact, that phosphoric acid, if heated on glass of inferior quality till it volatilize, will act upon it with considerable energy,* and all the animal substances in which fluorine has been said to exist, are particularly rich in phosphoric acid;—thus the ashes of ivory, of human bone, and the enamel of teeth, as also the precipitate obtained from urine by means of lime-water, are all of them composed, in very great part, of phosphate of lime.

Mr Richard Phillips has mentioned (in the Annals of Philosophy, Vol. v.), that when the water contained in uranite is driven off from the powdered mineral, a portion of the phosphoric acid is volatilized with it; the heat used being that of a common spirit-lamp. This is the only fact with which I am acquainted (with the exception of my own observation), to shew that phosphoric acid will volatilize with water. The heat used in Mr Phillips's experiment was, most probably, considerably higher than any which I applied. There seems no doubt that phosphoric acid is much more volatile than it has heretofore been supposed. Having failed in detecting fluoric acid in human bones, I determined on testing for its presence in the enamel of teeth, in recent ivory, and in the precipitate obtained from urine by the addition of lime-water. Two different specimens of ivory (tusks of the elephant) gave no evidence of the presence of fluoric acid, when carefully tested,

* It must be borne in mind, that the fluoric acid acts with facility on every kind of crown or flint glass, however good their quality may be. The supposition that bad glass was used in the experiment, is the only means I have of explaining away that which I feel sure is an error on the part of several continental chemists.

either before or after calcination; and I was equally unsuccessful with the enamel of human teeth, and the precipitate from the urine.*

In these experiments, when I had failed in acting on the glass, I always found that the addition of 0.3 grains of fluoride of calcium to the experiment, produced a strong and indelible mark on the surface of the glass test-plate. I mention 0.3 grains, because it will always be found sufficient to produce a most unequivocal corrosion, though I obtained satisfactory results by the addition of a much less quantity. I have had only one opportunity of examining fossil ivory; and in that instance, I could not ascertain its locality. On submitting it, however, to the test used for recent ivory, bone, &c., I obtained immediate action on the glass. In conclusion, I must express my firm conviction, that fluoride of calcium, as an ingredient in fossil ivory, must be regarded as an extraneous matter, introduced by the partial mineralization of the animal substance;—that no such constituent exists in recent ivory, the enamel of teeth, human bone, or urine;—in fact, that fluoride of calcium should be expunged from the list of the constituents of animal substances.

August 1839.

Sketch of Animal Life in Nova Zembla (Novaïa Zemlia.)

By K. E. Von Baer.†

The entire absence not only of trees, but also of every shrub which, without being sought for, might yet be sufficiently large to attract the eye, communicates to polar landscapes a peculiar and impressive character.

First of all, the power of measurement by the eye is lost, for, owing to the want of the usual objects of known dimensions, viz. trees and buildings, distances seem less considerable

* I was much pleased to observe that Mr Pepys, in an analysis of enamel, published in Mr Fox's work on the Teeth, does not mention fluoride of calcium as an ingredient of that substance. This analysis was made by Mr Pepys in 1833, several years after the fluoride had been declared a constituent of the enamel.

† From "*Wiegmann's Archiv für Naturgeschichte.*" 1839. Part II. p. 160.

than they really are, and even mountains appear to be of lower altitude. This remark has been often made and was not unknown to me, but, nevertheless, I found the deception, for which I was prepared, much greater than I had anticipated. I knew well, that from this cause alone, an expedition sent out by King Frederick the Second, from Denmark to Greenland, had failed in accomplishing its object. Mogens Heinson, who has always been regarded as an able seaman, commanded the ship, and, having come within sight of the coast of Greenland, steered directly for it with a favourable wind; but after sailing for several hours in the same course, thought that he approached no nearer to the shore. He became alarmed that some hidden power at the bottom of the sea detained him, and turned his vessel towards Denmark, whither he carried the report that having been attracted by a magnetic rock, he had not been able to reach the coast of Greenland. I was also acquainted with the *naïve* remark of Martens on Spitzbergen. "The miles appear quite short, but, when we come to walk them on land, they seem very different, and soon cause fatigue;" yet after all, the deception was so complete, that no reflection could remove it. I am likewise convinced that it depends not only on the want of the customary objects, but also on a peculiar transparency of the atmosphere, for on dull days it is not so perfect as on clear, and is not so striking in flat as in mountainous districts. On bright days, or at clear periods of the day, the air seems to be almost entirely colourless, and, as the heights visible to the eye are partly covered with snow, and partly exhibit a dark, and from the contrast, apparently a very dark coloured rock, the slight colour possessed by the air cannot be recognised. The mountains therefore seem to approach quite near to the spectator, and probably most so to those who have been accustomed to view mountains through a different kind of atmosphere.

Another effect of the want of trees, shrubs, and even grasses of considerable size, is the feeling of loneliness which seizes not only the man of reflection, but even the rudest sailor. There is nothing painful in this sensation; for it is of a solemn and elevating character, and can only be compared to that powerful impression which a visit to Alpine heights leaves in-

delibly fixed on the mind. I could not suppress the idea that suggested itself, of a comparison with the appearance of the morning of creation which was yet to be followed by the production of life. But, nevertheless, the movement of animals is occasionally to be witnessed in Nova Zembla. Sometimes a large gull *(Larus glaucus)* may be descried floating in the air, even at some distance from the coast, or a swift lemming running on the ground. Such occurrences, however, are not sufficiently frequent to give life to the landscape.

In still weather there is a want of sounds and of sufficient movement, when, as in our case, an expedition is made into the interior, after the departure of the numerous geese which moult on the lakes. The few land birds of Nova Zembla give forth no notes, and the comparatively even less abundant insects produce no noise. The polar fox is only to be heard during the night. This total absence of sound, which is more remarkable in calm weather, reminds the traveller of the stillness of the grave; and the lemmings, issuing from the earth, moving along in a straight line, and then speedily disappearing again in the ground, may be compared to spectres. Notwithstanding these signs of animal life, it really seems entirely awanting, owing to the small amount of movement visible. In other parts of the world we are accustomed to have the slightest breath of wind rendered apparent by means of the leaves of lofty plants and trees; but a gentle breeze has no effect on the diminutive plants of the high north—they almost look like painted representations of vegetation. There are almost no insects employed in satisfying their little wants upon them. Of the numerous family of the beetles only one individual was found, viz. a *chrysomela*, which is perhaps a new species. It is true that on warm days and in mild places, for example near little projecting masses of rock, a bee may be seen on the wing, but, as on moist days with us, no humming is to be heard. Flies and gnats are more abundant; but even these are so rare, and at the same time so quiet and dull, that they must be sought for in order to be remarked. I never heard of one of our party being bitten by a gnat, and it may even be said that a wanderer in those regions longs to be stung by a Lapland gnat, in order

to be assured of the existence of some symptoms of life in nature. But the most striking proof of the scarcity of the insect tribe, is afforded by the fact, that the carcass of a walrus, which had lain fourteen days on the coast, was found by us to be just as devoid of insect larvæ as the bones of animals killed on previous years, although portions of dried flesh were not wanting. Hence, in the extreme north, the common form of speech employed in our funeral sermons, that man becomes a prey to the worms, is not correct; and, whoever would wish to escape this fate, has only to be buried in Nova Zembla or Spitzbergen, where also the universal decomposing power of nature will act on him only with extreme slowness.*

The abundance or scarcity of insects is, after that of the vegetable kingdom, the best test of the climate of a district. Both require for their existence a certain quantity and a certain duration of heat. These conditions are never awanting in the torrid zone, but exist in a smaller degree as we approach the north; though it must be remarked, that insects are less easily transplanted than plants. This cause accounts for the total absence of true insects in Spitzbergen. Lehmann has noticed ten species in Nova Zembla, and of these, seven which are not parasitic. Fabricius described a much larger number from Greenland, and, among others, even several butterflies; and Scoresby has added several new species from East Greenland. But West Greenland, which is generally taken as the type of all the northern regions, because it has been rendered for a long time universally known by means of the Moravian missionaries, must, especially in the southern portion, be a far more favoured land, for it has—(not to refer to old fabulous accounts),—at the present day, in latitude 61° N., birches of twelve or eighteen feet in height, and as thick as a man's leg, with intermixed mountain-ashes.—(*Egede, Nachricht von der Grönländischen Mission*, p. 78.) Egede found that corn, which he had sown as an experiment in 64° N. latitude, was on the 13th September not only in ear, but also provided with small grains (pp. 106 and 112). Hence it is evident

* At a certain depth, bodies remain in a frozen condition, but even above the earth they are preserved for a remarkable length of time

that the region thus described by Egedé is very different from Nova Zembla, and observations made on the weather prove beyond a doubt that there is a much greater degree of warmth. It is however, also to be noticed, that some regions which have a much lower mean temperature than Nova Zembla, are much more prolific of life, if there only happen to be more heat in summer. In order to select an example which is less known, I would adduce Nyshne-Kolymsk which has a mean temperature of —10° C. (= + 14° Fahr.). According to Wrangell's observations, the boundary of tall trees is not far distant, and would perhaps reach this place were it not for the proximity of the coast, for at Nyshne-Kolymsk there is abundance of stunted Siberian cedars and of shrubs. There the gnats are an unendurable torment during the short summer.

The coast of Nova Zembla is much more animated than the interior, owing to the number of sea birds which there build their nests. Their number and variety are certainly not so great as on the Norwegian coasts or on some islands and cliffs of Iceland, but still the sea-shore is thickly peopled by them in some places, on approaching which the traveller is received with a loud noise. The Foolish Guillemot *(Uria Troile)* especially, whose abundance equals that of all the other birds taken together, lives in colonies of this description. This bird is to be seen in crowded groups, and in densely packed rows ranged one above another, which are placed on hardly perceptible projections of the perpendicular rocky walls; they turn their fronts towards the approaching stranger, and the dark precipice seems spotted with their white bellies. The Russians term such brooding places bazaars. Thus has this Persian word been transplanted by the Russian walrus-fishers to rocks of the Frozen Ocean, and in the absence of human beings applied to the feathered race. The great grey gull *(Larus glaucus)*, named by the Dutch fishers, either from respect or want of it, the Burgomaster, builds its nest on the summits of isolated rocks, and allows no other bird to approach it. It seems to regard itself as the lord of this creation, for it has confidence enough, in the presence of a whole party of the fishers, to carry off fish that have been thrown by them on the sea-shore.

These birds are the best proof that more is to be obtained from the bottom of the sea than from the dry land. In fact, the great mass of animal life is here buried under the surface of the ocean. Small crabs are particularly abundant, and more especially Gammari, which surround a piece of flesh thrown into the water, almost in as great numbers as the gnats which collect about a warm-blooded animal in Lapland. They may be obtained in thousands by means of a sieve. When we threw our lines in Matotschkin-Schar, we were assured by the walrus fishers, who never give themselves this trouble, that our trial would be a vain one, as in the first place there are almost no fish, and in the next place sometimes the bait and sometimes the fish are completely consumed in a few hours by the *Kapschaki*, as these Gammari are termed. We soon found, indeed, that we rarely drew up anything with our lines.

Although the vegetation is so sparing, yet it supports a multitude of lemmings. Gentle acclivities are frequently burrowed by them in all directions. But still the number of these creatures is not nearly so great as one would be led to believe by the multitude of excavations; for most of them are empty, as is easily proved by the use of dogs; but, nevertheless, their number is so considerable as to make us ask ourselves how so many lemmings can subsist on so meagre a vegetation. It is by no means impossible, that the vegetation appears so poor, because the lemmings render a large portion of it invisible. Were they to devour the roots, there would soon be but little of the vegetable kingdom remaining in Nova Zembla, and the lemmings would soon become extinct from the absence of nourishment; but those caught by us could in no way be brought to eat the smallest root. As, in their natural state, they only eat flowers and green parts, and as all the plants are perennial, the latter produce new stems in the course of each succeeding year. It seemed still more remarkable to us that, even when in the greatest state of hunger, they would not touch cryptogamic plants. I regretted that the small number of ferns did not permit me to try the experiment, if these practical vegetable phytophagists regulate themselves by the presence of the spiral vessels, or by the Linnæan principles of classification. The lemmings are of two

species: the one seems to be the *Mus grœnlandicus* of Traill, or *Mus hudsonius* Auct.; and corresponds exactly with the description published by Richardson in his "Fauna boreali-Americana," though it agrees less completely with the account given by Pallas; and the other species appears to me to be also distinct from the Scandinavian lemming, indeed the colour is strikingly different, but Pallas, who seems to have had only young specimens before him, considers it as the Russian variety of the Scandinavian lemming. The first is remarkable for its tameness, for, four and twenty hours after being captured, it hardly attempted to escape when held freely in the hand; and two individuals of that species are never seen quarrelling with each other. The other or yellowish-brown species is much more quarrelsome.

Though not so abundant as the lemmings, Polar foxes are rather common. They find abundant food in lemmings, young birds, and the animals thrown ashore by the waves. Polar bears are rarely seen in summer, either because they avoid places where they scent human beings, or because they only collect at those points on the coast where there is ice. The rein-deer, also, owing to the number of walrus-fishers who pass the winter there, have become scarce, at least on the west coast, during the last few years. Not only were but very few killed during our stay in the country, but one of the parties who had spent the previous winter in Nova Zembla, and had been instructed to support themselves by hunting the rein-deer, had not been able to obtain any. Wolves and common foxes, which occasionally occur, at least in the southern half of Nova Zembla, seem never to have been numerous. With the above enumeration, the notice of the land mammalia would be complete, were it not that Messrs Pachtussow and Ziwolka, during their winter residence in a hut, saw a little white animal, which they call a mouse in their journal. As the animal, according to Ziwolka's account, was larger than a common domestic mouse, and therefore could not be an individual of the white kind which had been taken there accidentally by a ship, I am doubtful what to make of the observation. On the one hand, it has been said that the North American lemmings become white in winter, but still not so

white as animals of the weasel genus; and, on the other, it is also possible that the animal was a weasel. In Spitzbergen, also, a little white mammiferous animal has been noticed, whose exact nature is undetermined.

The sea mammalia are more important; for, in their pursuit, many expensive expeditions are yearly fitted out by the inhabitants of the White Sea; whose success, however, is unfortunately so precarious, that they may be truly compared to a game at hazard. When the sea is unusually free of ice, the losses are very great; but the success of a single day may make up the loss of a whole year. For this reason these hunting undertakings have been renewed year after year for centuries, although they are sometimes complete failures. The usual consequence of a lucky year is, that, on the following one, many ships proceed to Nova Zembla, and that either too great havoc is committed among these, for the most part, gregarious animals, or at least they are frightened away. Thus in 1834, after some years of intermission had occurred, the speculation was an extremely good one; and in the year 1835, about 80 ships sailed to Nova Zembla, whose united amount of men must have been at least 1000. In 1834, the number fell one-half. In the current year* there were not much above 20 ships, and only one, which entered the Kara Sea, obtained great success; one or two got enough to pay their expenses, but most of the remainder lost more than half the cost of their outfit.

The most valuable animal in these marine hunting expeditions is the walrus; and next to it is the dolphin (*Delphinus leucas*), termed the white whale, but which there receives the name of *Bjelucha* or *Bjeluga*. Among the seals, the species which affords the richest return, both on account of its size and its abundance of fat, is the sea-hare (*Morskoi sajaz*) *Phoca leporina*, Lep. *Phoca albigena* Pallas, but probably not different from the *Phoca barbata* of Fabricius. The *Phoca grœnlandica*, according to its age and sex, receives very different

* M. Baer descended the river Dwina from Archangel on the 19th July 1837, and returned to Archangel on the 11th September of the same year.—Editor.

names from the Russians; the old male is termed *Luisan* or *Luisun*, the female *Utjälga;* the young ones of a year old, whose colours are not completely developed, *Sjärunok* and *Sjärka;* and the young ones, according to their various tints, *Pljächanko*, *Choçhlutschka*, and *Bjäka*. But the names of the young animals are not always strictly applied, for the same denominations are given to the young of a third species of seal which occurs here, and of which the old ones receive the name of *Nerpa*. This species, of which single individuals occur every where along the coast, is most likely the *Phoca hispida* of Fabricius. A fourth species of seal belongs to these seas, though it is not actually met with on the coast of Nova Zembla; but occurs on the Timanic coast and in the entrance to the White Sea, though not abundantly. It is the *Tewjak*, is said to cover its face with a sort of a cap, and is therefore very likely the *Klappmüts* of the Dutch, or *Phoca cristata* Erxl., *Cystophora borealis* Nilsson.

Of the cetacea, this sea contains, more especially, a species of whale belonging to the subdivision termed Fin-fish or Balænoptera, with very short whiskers, and which I saw at Archangel. They are seen but rarely in the sea round Nova Zembla, and one hears nothing of any being stranded on the coast. Nearer the north coast of Lapland, where they are stranded almost every year in the Bay of Motowsk, they are so numerous, that I am much surprised that earlier attempts have not been regularly followed up, and new enterprizes carried on with perseverance for the pursuit of this animal. It may be at the same time remarked, that it is undoubtedly difficult to kill this animal. It is extraordinary that the Greenland whale never strays to the coasts of Nova Zembla. This is a strong reason for believing that the whale-fishing carried on by the Northmen, in the ninth century, near the North Cape, according to the testimony of Onthere, must have been for this fin-fish. The narwhal (*Monodon monoceros*) is much rarer, and is only met with near the ice. As to dolphins, besides the *Delphinus leucas*, there is also the *Delphinus orca* (*Kossatka*), and a small species termed by the Russians *Morskaja swinja*, regarding which I have not been able to ascertain whether it is the *Delphinus delphis* or *Delphinus phocæna*.

The marine mammiferous animals of Nova Zembla, would therefore be the same as those known to occur in the Spitzbergen and Greenland seas, if the Greenland whale extended so far. On the other hand, Spitzbergen and Nova Zembla differ in a striking manner in their feathered inhabitants. The latter indicates by its birds the proximity to a continent. It is richer in species, but is at the same time less interesting for the naturalist, as many of these species are the same which yearly visit us, and partly remain with us, but of which another portion proceed to Nova Zembla, in order to pass their breeding season in undisturbed tranquillity. Of land birds we found the Snowy Owl *(Stryx nyctea)*, which even passes the winter there; the Snow Bunting *(Plectrophanes nivalis)*; *Strepsilas collaris;* the Striated Sandpiper *(Tringa maritima)*; and a Hawk which was by no means rare at Kostin-Schar, but which could not be obtained, and therefore was not more exactly determined. Old accounts mention also an Eagle, but the walrus fishers, of whom I inquired, could give me no information about it; and it is probably in fact the above-mentioned hawk.

As to the web-footed birds which here pass the season, the Wild Geese *(Anas ferus* or *segetum) (Saatgänse)* are so numerous, at least in the southern island, that the collecting the wing feathers which have fallen out is an object of traffic; the Long-tailed Duck *(Anas glacialis)* is also common, and the *Cygnus musicus* is not rare.

The walrus hunters maintain that there is only one kind of goose in Nova Zembla, and in fact we saw no other but the Wild Goose and the *Anser torquatus*, which last, however, is not considered as a goose by the Russians. The Eider-duck or Eider-goose is also not of rare occurrence. But the web-footed herbivorous birds are not nearly so abundant in Nova Zembla with its miserable vegetation, as on the island Kolgujew, which is represented to be covered with geese and swans. Expeditions are sometimes sent there for the purpose of killing and salting them. On one occasion in the course of two hunts no less than 15,000 geese were slaughtered, as I was assured by a merchant of Archangel.

Among the web-footed birds of Nova Zembla are also to be

enumerated *Uria Troile* (in prodigious numbers), *Uria grylle*, *Colymbus septentrionalis*, *Sterna hirundo*, *Larus glaucus*, *Larus canus*, *Larus tridactylus*, *Lestris catarractes*, and a *Procellaria* which we were not able to procure. *Somateria spectabilis* and *Larus eburneus* are said to be met with only on the northern coast. There also occur, according to the descriptions given us, *Mormon fratercula* and *Mergulus Alle*. It struck me as being extremely remarkable that no one has seen a bird of the Auk family to the south of Kostin-Schar, for *Alca pica* by no means belongs to the birds of extremely northern regions, and *Mormon fratercula* occurs on the Norwegian coast.

There is no trace of the class amphibia in Nova Zembla. Batrachian and saurian animals evidently cannot exist there from the want of insects.

The High North presents but few species of fish, even where individuals abound. Scoresby enumerates only four species of fish as occurring in Spitzbergen and the neighbouring seas. My list of the fishes of Nova Zembla includes 10 species, all of which we saw except the Omul (*Salmo omul*, Pall.), which is said to be found on the West coast. The most important of these is the *Golez* (*Salmo alpinus*, Fabr.), which ascends to the mountain lakes in autumn, and in some years is taken in immense quantity and carried away. All the other fishes are either unimportant or useless for commerce; and, in the economy of nature, the only two that seem of much consequence are the *Gadus saida*, Lep. and *Cyclopterus liparis*.

Experiments on the Temperature of Vegetables with the Thermo-Electric Apparatus. By M. Dutrochet.*

Vegetables, in addition to a peculiar heat of their own, have also that of the atmosphere. This heat is totally absorbed by the vaporization of the sap, by the production of oxygen during the day, and carbonic acid during the night.

* Communicated to the French Academy of Sciences, in June 1838, and read in June 1839.

Hence it follows, that vegetables, in their natural state, have a temperature always lower than that of the atmosphere, and thus appear *to produce cold.* Among the cooling causes of vegetables, the vaporization of the sap is most conspicuous. In proof of this, it is only necessary to place the vegetable in an atmosphere completely saturated with water. This I have done by means of a particular apparatus. It is a large glass vessel, closed with a cork, and in which is a little water. The two soldered points of the thermo-electric circuit are situated in the interior of this vessel. One of these is occupied by a vegetable killed by immersion in hot water; the other by a vegetable exactly similar, but alive. The needles are covered with gum lac, to preserve them from oxidation and the action of the acids. The two vegetables, in the interior of which are the two soldered points, are thus placed in the same condition with respect to the external causes capable of modifying their temperature; the dead one takes the temperature of the surrounding atmosphere; that which is alive takes the same temperature, but in addition, it then manifests the heat possessed by it in virtue of its principle of life, and which, in its natural state, was absorbed by the vaporization of the sap. The maximum of this heat is $\frac{1}{4}$ of a centesimal degree; generally, however, it is only $\frac{1}{6}$, or even $\frac{1}{10}$ or $\frac{1}{12}$, of a degree. This I have observed in the young stems of vegetables whose pith is still green, in flower-buds, and in leaves sufficiently thick to admit the needle, such as the leaves of the house-leek (*Sempervivum tectorum*). The heat peculiar to young stems and leaves, disappears during the night, or artificial darkness, and reappears under the influence of light sufficiently long continued. The peculiar heat of flower-buds continues during the night. At first, I believed this heat disappeared during darkness, but afterwards discovered my error. The heat peculiar to vegetables is increased in proportion to the elevation of the external temperature; to have marked results, therefore, it is necessary to make these experiments above + 59° Fahr.; they are still more appreciable above + 68° Fahr. The highest elevation of the heat peculiar to vegetables, corresponds to that hour of the day when the external heat and the light are both at their

maximum. Now, as it is then that vegetables produce the greatest quantity of oxygen which is introduced into their pneumatic and respiratory organs, it follows that their production of heat is in proportion with their respiration, in a similar manner as with animals. Finally, the heat which vegetables manifest when surrounded with an atmosphere saturated with water, is only a part of the total heat which they produce, since another portion is necessarily absorbed by the production of oxygen under the influence of the light. All my experiments were made in diffuse light.

Additional Note on the Temperature of Plants. By M. Dutrochet.*

The delay of a year in publishing these observations, was caused by the fear of having been led into error by an instrument which is the source of many deceptions, against which it was necessary carefully to guard. The thermo-electric apparatus I employed last year, gave 6° of deviation of the magnetized needle, for one centesimal thermometric degree of difference of temperature between the two cemented points; this year, furnished with an excellent galvanometer of Gourjon, I obtain 16° of deviation of the magnetized needle, for one centesimal degree. Provided with so sensible a thermo-electric apparatus, I have repeated, with less chances of error, and with much more precision, my observations of last year, and have fully verified their accuracy. The apparatus I used, and a figure of which I have given, is only proper for cut plants, whose life is maintained by the cut end being immersed into water. This year I employed a more perfect apparatus, with which I can subject to observation not only cut plants, but also plants still growing in the ground. I will give, in the memoir I am about to publish, a description of this new apparatus, constructed on the same principles as the former, but in which the glass vessel employed formerly has been replaced by a high cylindrical bell-glass. My needles are differently constructed from those I used last year. They are made of very fine copper and iron wires, bent back on themselves, at a very acute angle, where they are soldered

* Communicated to the French Academy of Sciences, in June 1839.

together, so that the soldering occupies the point of the angle, the two sides of which, the iron and copper, are almost in juxtaposition, and separated only by a layer of varnish, which covers all this part of the needles. The soldered point is thrust also to the same depth of five millimetres into the part of the vegetable submitted to observation.

I will present here some modifications and additions to my observations of last year, already explained.

The heat of the green parts of vegetables, a heat the maximum of which I have stated to be $\frac{1}{4}$ of a centesimal degree, is sometimes elevated to $\frac{1}{3}$ of a degree. This I have observed in the stem of the *Euphorbia lathyris*. Stems discover a peculiar or proper heat only in their herbaceous state; they lose it on becoming ligneous, or at least it is not then appreciable. I have ascertained the existence of vegetable heat not only in the parts I have mentioned in my note of last year, but also in roots, in fruits, and even the seminal embryos. It is easily observed in the large cotyledons of the bean (*Vicia faba*), while they are still green. I have observed it also in mushrooms. Flower-buds discover a peculiar heat only when the soldered point is placed in the ovary. When it is placed in numerous petals folded back and pressed together, as happens in the flower-buds of the hundred-leaved rose, or the double peony, no indication of heat is observed. The peculiar heat of young stems, which often disappears during the night, sometimes continues during this period, but it then experiences a remarkable diminution. It is usually in the course of three hours after mid-day that vegetable heat is at its maximum. This is the period of the day when *ordinarily*, but not *always*, the heat and light are greatest. Beyond the invariable hour at which the maximum of the peculiar heat of each plant takes place, this heat goes on diminishing, although the external heat and light may then augment in intensity. This *diurnal paroxysm*, this sort of *quotidian fever*, which green vegetables experience, presents no interruption, except during the complete absence of the diurnal light; and, what is very remarkable, this interruption of the paroxysm does not always happen on the first day of total obscurity. I have seen this paroxysm return on the first, and even sometimes on the second day of darkness, and its maximum always happened at

the same hour. On the third day of obscurity, the paroxysm did not return. Exposure to simple diffuse light, is all that is necessary quickly to restore to the plant the proper heat it has lost.

I wish to compare the proper heat of vegetables with that of some animals of *low temperature*. I make use of this expression because I do not think that there are any *cold* animals, that is, absolutely deprived of a proper heat. I have found that the heat of vegetables is generally little inferior to that of insects, and is often superior to it. The proper heat of the frog *(Rana esculenta)* is much inferior to that of most plants. I have found no appreciable heat in the crawfish *(Astacus fluviatilis)*, nor in the slug *(Limax rufus)*. Thus vegetables are placed higher in the scale than certain animals with relation to the degree of their proper heat.

M. Becquerel having stated that he found the temperature of the living branch of a tree several degrees higher than in a dead one, M. Dutrochet, in a subsequent note (July 1st) remarks: When I formed the plan of making investigations regarding the temperature of plants by the assistance of the thermo-electric apparatus, I consulted M. Becquerel about the mode of employing this apparatus; and he at once communicated to me his manner of operating, with that frankness which characterizes the true man of science. He informed me that, conjointly with M. de Mirbel, he had made the experiment, at that time unpublished, of which an account was given in the notice to which this is intended to reply, and that the result of that experiment had been to enable him to discover in a branch of a living tree which contained one of the soldered points of the thermo-electric circuit, a temperature some degrees higher than that possessed by a dead branch which contained the other soldered point, and which was estimated to be of exactly the temperature of the atmosphere. If the result of this experiment had been beyond the shadow of suspicion of error, there would have been no doubt of Messrs Becquerel and Mirbel having ascertained before me the existence of a heat proper to the branches of plants, a heat long sought for and at that time unsupported by evidence. I had doubts of the certainty of the result. In the trunk and in the branches of a tree, there is constantly flowing the ascending sap, which

supplies with rapidity what is lost by the evaporation of the leaves. This sap, in passing from the roots into the trunk, carries along with it the temperature of the ground. This temperature is modified in the trunk by the temperature of the surrounding atmosphere, and often by the direct action of the solar rays, so that a different temperature is found to exist in the trunk of the same tree according to the height where it is observed at the same time. The trunk retains for several hours this acquired heat after it has ceased to exist in the atmosphere, and the sap which traverses it in its ascent to the branches, conveys to them the temperature which it there acquires. Here, then, is a source of error which it is impossible to avoid in the investigation of the heat proper to the trunk and the branches of trees. It was not, therefore, to these that I directed my researches. Thinking that the heat proper to plants, if it existed, ought to be found more easily in the soft parts, where life is active, than in the hard portions where life possesses less activity, it was to the former that I purposed to devote my special attention.

The Sorel apparatus, intended for procuring a constant temperature, to which one of the two soldered points of the thermo-electric circle is submitted, cannot be employed in this kind of investigation. M. Becquerel formed the happy idea of supplying its place by inserting one soldered point of the circuit in a living branch, and the other in a dead branch of the same tree; both branches having the same dimensions. It was evident that these two branches, on account of their equality, ought to partake simultaneously of the variations of the temperature of the surrounding atmosphere; so that, if the living branch had a heat proper to itself, it ought to add it to the heat transmitted from without, and to manifest its excess of heat above that of the dead branch by a deviation of the magnetic needle of the multiplicator. I hastened to follow this plan of experimenting. My first experiment was made on a young branch of *Campanula medium* which I cut, and which, having its interior extremity plunged in a vessel full of water, was carried to my room. One of the soldered points was placed in its interior, and the other in a branch of the same plant which had been dead and dried since the preceding year, and which was of the same size. The constant

result of this experiment was to indicate to me a higher temperature in the dead branch than in the living one, and that with irregular variations in intensity; I observed the deviation of the magnetic middle from hour to hour. The following day I replaced the dry branch by a green branch of the same plant, but one which I had deprived of life by plunging it for five minutes in water heated to 50° (122° F.), a heat not sufficient to boil it in any degree. I then left it to cool. In this second experiment I obtained an opposite result, for it was invariably the living branch which exhibited the greatest degree of heat, and that with irregular variations. The contradictory results of these two experiments, gave me reason to think, that the cooling produced by the evaporation of the fluids contained in these branches, was the cause of the strange differences which manifested themselves between their reciprocal temperatures. The living branch being placed in comparison with the dead and dried one, the first experienced, owing to the evaporation of its fluids, a cooling which the second by no means suffered, so that the latter possessed the largest amount of heat.

When the living branch was placed in comparison with the dead one, which was still filled with its sap, these two branches were cooled unequally, owing to the unequal evaporation of their fluids; an evaporation much more considerable in the dead branch than in the living one, so that the latter ought thus to manifest a higher temperature. I have indeed found, that under the influence of the same external causes, the evaporation is more considerable in the dead branches than in the living ones of the same dimensions. This fact proves, that these living vegetables exercise an action which tends partly to remove a part of their organic liquids from the dissolving action of the atmosphere. The living branch only gives up to evaporation what it exhales, a phenomenon both physiological and physical; while the dead branch gives up its liquids to evaporation, just like a moistened piece of cloth, a phenomenon purely physical. M. Becquerel does not say in his note if the dead branch which he employed in his experiment was dried, or if it still possessed a part of its sap. It appears to me very probable, from the result of his experiment, that the latter is the truth. But, I return to my two experiments

mentioned above, and whose results were contradictory; they proved to me, that there was a source of error in the employment of the mode of experimenting pointed out to me by M. Becquerel; a mode of experimenting, whose principle nevertheless was good, but which required an addition. The object was to put an end to the evaporation which was an incessant cause, variable and unequal, of cooling for the two branches; and I obtained it by placing these two branches, the one dead and the other living, and both green, in a large matrass, closed by a cork, at the bottom of which there was a little water intended to saturate by its evaporation the air contained in it, and to support the life of the plant plunged into this liquid at its lower extremity. The evaporation of the fluids contained in the two branches being thus suspended, and consequently this cause of cooling no longer existing, the heat proper to the living branch exhibited itself; and it must not be supposed that it is the unequal evaporation of the living branch which, continuing to take place in the close vessel, was the cause of the greater degree of heat manifested by this last, for its higher temperature exhibited itself in the same way when the dead branch, full of its organic fluids, was replaced by a dried branch. However, I could not with confidence have made use of dried branches in my experiments, because these branches, in the cells and vessels of which the air had replaced the organic liquids, ought on that account to be less easily permeable to heat than the living branches full of fluid, so that they were not suited to acquire at the same time the variations of the surrounding heat. I could not thus hope from their employment results so exact as those which might be afforded me by using green branches deprived of life.

It will thus be perceived what I owe to the advice of M. Becquerel, and I am glad to acknowledge my obligation; but he will, I hope, agree with me that the process of experimenting, such as he recommended to me, and such as he employed himself, carried along with it causes of error. I am not afraid therefore to affirm, that it is by the effect of these causes of error that he has found in a living branch of a tree a temperature higher by some degrees than that presented at the same time by a dead branch. The heat of branches is

never so much elevated, for, at its maximum, it only reaches one-third of a degree, according to my observations. Besides, I must here add, that numerous experiments have proved to me, that it is only the branches in a soft or herbaceous state which have heat proper to themselves. I have never found the slightest trace of proper heat in the ligneous tissue of trees; and my thermo-electric apparatus indicates, without difficulty, the existence of $\frac{1}{64}$ of a centesimal degree of heat, corresponding to $\frac{1}{4}$ of a degree of deviation of the magnetic needle of the multiplicator. I have observed the proper heat of young branches of many trees or shrubs such as the *Aylanthus glandulosa*, Desf. and the common elder (*Sambucus nigra*), branches having a sufficient size, in their herbaceous portion, to be submitted to this kind of experimenting. This herbaceous part, that is to say the upper *merithalles*, alone exhibited a proper heat; I found no trace of it in the lower *merithalles*, whether the soldered point was placed in the pith or in the ligneous tissue. Of course, I found no proper heat in the ligneous tissue of the older branches.

To conclude, I do not hesitate to affirm, that the heat perceived in the branch of a tree by Messrs Becquerel and Mirbel was not the proper and vital heat of that branch: whence it follows that no one before me has demonstrated or even ascertained the existence of this vital heat in the branches of plants, for I regard as null the researches which have been made on this subject by placing thermometers in holes formed in the trunks of trees.*

* On the 18th November 1839, M. Dutrochet read a notice containing an account of new researches which he has made with the thermo-electric apparatus, on the vital heat of plants. They confirm the observations communicated in the preceding paper. Thus, it would appear certain, that all plants have a proper and vital heat; that this heat exists especially in the green parts; and lastly, that it undergoes a quotidian paroxysm, which reaches its maximum during the day, and its minimum during the night. The following are the most striking facts contained in the new communication: The *Euphorbia lathyris* is the plant, a stalk of which has afforded the greatest degree of vital heat. The hour of the quotidian maximum is always about the same for each plant. This hour varies from 10 o'clock in the morning till 3 o'clock in the afternoon, in different plants. No proper heat has been detected in ligneous tissue, even in that of recent formation. Complete darkness does not prevent the reproduction of the diurnal paroxysm of the proper heat of the stalks of plants, and that during some days; but it goes on gradually diminishing its intensity, till the complete extinction of this vital heat.

Register of Mean Temperature, &c. at Applegarth Manse, Dumfriesshire, Long. 3° 12′ *W.; Lat.* 55° 13′ *N.; Height above the Sea* 180 *feet; distance from the Sea* 10 *miles. Times of Observation* 9 A. M. *and* 9 P. M. By the Rev. WILLIAM DUNBAR, D. D. Communicated by the Author.

Years.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Ann. Mean.
1827,	34.25	31.75	38.16	46.30	51.47	57.25	63.83	60.00	57.00	51.38	41.19	41.50	47.84
1828,	38.25	38.54	41.06	44.08	52.75	57.75	59.67	57.66	55.16	47.60	45.08	44.61	48.52
1829,	30.00	38.12	38.37	42.34	56.50	58.60	58.33	55.90	50.20	45.50	39.50	35.50	45.73
1830,	32 50	33.75	43.00	46.52	52.04	53.80	58.90	55.22	52.10	48.24	42.22	34.40	46 05
1831,	33.00	36.10	42.12	46.75	52.25	60.30	60.30	60.75	54.80	52.50	39.15	39.90	48.16
1832,	36.10	37.50	40.02	46.20	49.80	57.05	58.65	56.05	54.60	49.15	38.25	38.05	46.78
1833,	31.05	38.15	38.02	43.70	57.95	55.50	59.45	55.50	52.60	47.25	42.35	39.20	46.72
1834,	40.37	39.00	40.87	44.99	52.23	56.66	60.50	56.87	52.25	47.00	40.12	39.38	47.52
1835,	33.00	38.13	37.75	43.50	48.50	56.12	56.63	58.50	51.25	43.75	40 50	35.75	46 28
1836,	36.30	33.75	37.15	40.90	50.80	54.00	54.05	53.70	48.70	42 75	36.55	35.45	43.67
1837,	32.50	40.50	32.00	37.20	47.30	55.05	58.80	54.75	50.65	46.25	35.90	37.40	44.02
1838,	27.90	28.80	36.50	39.30	47.90	53.80	57.10	54.80	51.08	46.10	37.25	37.30	43.15
Month. Mean,	33.67	36 28	38.55	42.19	48.55	54.29	57.29	56.64	51.32	46.45	40.15	37.47	46.37

Rain in Inches. Gauge 5 *feet from the Ground.*

Years.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Ann. Mean.
1827,	3.63	1.07	3.07	1.88	2.40	2.75	1.88	3.40	3.13	2.90	3.25	5.34	35.96
1828,	3.88	2.20	1.13	2.63	1.40	2.10	7.50	5.75	2 75	3.00	3.20	4.50	40.04
1829,	0.25	1.13	1.33	3.50	1.25	4.00	2.63	4.50	3,75	5.00	2.50	1.50	31.34
1830,	0.20	1.80	1.80	3.30	2.60	1.20	4.40	2.70	6.50	1.50	5.70	1.70	33.40
1831,	1.75	2.25	3.90	1.20	0.13	2.90	2.60	2.20	3.50	7.25	3.70	4.25	35.63
1832,	2.00	3.50	2.70	1.50	1.50	2.10	0.90	1.60	0.60	5.50	4.50	3 30	29.70
1833,	0.17	3.20	1.50	1.70	2.20	3.90	0.90	1.30	1.50	2.60	6.00	6.00	30.97
1834,	6.63	2.75	2.30	0.50	1.30	4.00	2.75	4.10	3.00	3.20	3.00	2.20	35.73
1835,	3.00	2.50	2.50	0.30	3.00	2.00	2.50	2.30	6.50	3.00	3.80	2.20	33.60
1836,	3.80	1.00	3.00	2.10	0.00	4.80	5.05	3.50	2.60	2.50	5.10	4.40	37.85
1837,	2.40	1.10	3.50	2.00	1.40	3 00	5.80	2.70	2.00	3.30	3.70	4.30	35.20
1838,	0.80	0.20	2.00	1.10	2.78	6.26	4.93	6.03	2.70	4.34	4.57	2.87	38.78
Month. Mean,	2.375	1.908	2.394	1.809	1.663	3.250	3.486	3.340	3.210	3.674	4.035	3.546	34.85

In the October Number of this Journal was given a Table of the mean temperature of Orkney. It may not be uninteresting to contrast with it the mean temperature of a district at the opposite extremity of Scotland. The accompanying register will shew, that, while the annual mean is nearly the same, the temperature of the former region is, from its insular position, more equable, the difference between the winter and summer months being less than in the former.

Researches on the Structure of the Scales of Fishes. By Dr L. Mandl.*

Chap. I.—*Historical.*

The researches made by authors respecting the scales of fishes have in general been confined to the examination of a very small number of them; they were merely observed through a magnifying glass, and their forms imperfectly described, no one entering upon the subject of their internal structure. M. Agassiz, when directing attention to them as affording important aids for the classification of fishes, is, in like manner, contented with merely determining their forms.

It is long since Borellus (*Petrus, observationum microscopicarum centuria*, Hag. Com. 1656) observed that these scales presented concentric lines divided by rays and black points. "Squamæ piscium apparent si aspiciantur, lineis orbicularibus multis distinctæ, et in parte qua cuti adhærent, radiis ac punctis multis transcurrentibus divisæ." The figure he gives of a scale entirely corresponds to this imperfect observation, the date of which may be said to be in the very dawn of micrography.

Hooke (in his *Micrographia*, London, 1667, p. 162) gives a figure of the scales of the sole, more accurate than those of any author that has succeeded him. Guided by our own researches, we can easily recognise in his figure the form of the teeth and longitudinal canals. His description of these details is very brief; he thus expresses himself in regard to the teeth:—"Through an ordinary single magnifying glass, they looked not unlike the tiles on a house," and he adds that they are "transparent and hard-pointed spikes." The longitudinal canals are "small quills or pipes, by which, perhaps, the whole may be nourished."

The observations of Leuwenhoek are among the most incomplete which that author has made; yet they were accepted by all his successors. He says (Cont. arc. nat. Lugd. Batav. 1722, (*Op. omnia*, p. 3) Ep. 107, p. 191, 1696; Ep. phys. Delphis, 1719; Ep. 24, p. 213, 1716) that a new scale is

* From Ann. des Sciences Naturelles, Juin, 1839.

formed every year below the old one, which forms a margin round it, so that we perceive on the scale the margin of the old scale; and that we may, in this manner, by counting the number of layers in a transverse section, determine the age of the fish, and the number of accessory scales which compose the entire scale. We shall afterwards point out the erroneousness of this opinion, merely remarking for the present, that the drawing which Leuwenhoek gives for a transverse section, represents nothing more than a part of the surface, and by no means the interior of the scale. This author had previously started another opinion (Arc. Nat. (*Op. omnia*, t. 1) Lugd. Batav. 1722, p. 105, 1685), the discovery of the scales of eels, of which he has furnished a better figure than any of his successors (*Opera omnia*, t. i. p. 48) and which he believed composed of globules; and some investigations relating to the perch, had led him to believe that a scale grows like wood, that is to say, that it forms a new circle every year. This opinion he subsequently abandoned, as we have just seen.

Reaumur (*History of the Academy*, 1716, Paris, 1718, p. 229) had already remarked that the silvery substance observed by the microscope or a powerful glass, is composed "of plates, the greater part of which are cut in a square form. These plates form rectangles about four times longer than broad; some of them, however, have their extremities rounded, and others terminate in points. They are all extremely thin." This author believed that these crystals could not be broken, an error arising from the circumstance of his having taken fragments for entire crystals. He affirms that these crystals are contained in vessels or a kind of tubes, mistaking the bundles in which they are naturally disposed for vessels. Reaumur believes, moreover, that the formation and growth of the scales of fishes is owing to this matter. If the *essence d'Orient* becomes putrid during summer, it is, he assures us, on account of the animal parts accidentally found there in solution, the crystals, according to him, being unalterable. This latter observation has not been confirmed by our researches. He also gives us a detailed description of the manner in which false pearls are formed by means of this matter, the presence of which on the lower side of the scale he very dis-

tinctly points out. These observations of Reaumur have escaped the notice of most of the modern authors who have written on the scales of fishes.

Roberg (*Dissert. de Piscibus*, Ups. 1717, in quarto), in the description he has published of the eel, has copied Leuwenhoek's figure.

Petit (*Memoires de l'Academie*, 1733, Paris, 1735, p. 193) gives only very incomplete figures of the scales of the carp, examined by the naked eye.

Schœffer (*Piscium Bavarico-ratisbonensium pentas*, Ratisb. 1761, p. 28, 43, &c.), in describing the scales of the fishes belonging to the family of the Percoides, alludes to the small teeth on their free edges, and the roughness resulting from this, in which he even thinks he can find the reason why pikes always swallow perches with the head foremost and never the tail:—"Omnes ex parte posteriori . . denticulis acutis exasperatæ; quæ tamen denticuli seu aculei minimè, ut auctores affirmant, retrorsum flexa, sed recta caudam respiciunt, quæ directio etiam in causa est, quæ manum caudam versus ducens nullam, caput versus punctoriam, asperitatem sensit;" and in a note,—"si id quod Willughbeius refert, perpetua experientia docet, quod piscis lucius percas semper capite, nunquam in cauda apprehendit et sic deglutiat, eo confirmaretur; lucium sibi mirum in modum providere, non inscium, a capite ad caudam denticulos non sentiri, et innocuos esse." He subjoins a plate containing figures of the scales of the perch, and although all these figures are alike, Schœffer thinks he can discover important differences in them.

Baster (*Opuscula Subseciva*, Harlemi, 1759–1765. Liber. iii. 1761, p. 127) treats of many different matters in a paragraph entitled *De Squamis Piscium*, and in the few words he devotes to the subject, he scarcely says more than Borellus. He adds a plate, however, containing figures of forty-one scales of different genera of fishes, among which we remark that of the eel, which Leuwenhoek had delineated about fifty years before with much greater accuracy. With regard to the small teeth of the scales, they are much more faithfully represented in Hooke's drawings, imperfect as these are.

It is important in the history of micrography to be made

sensible, by such examples, how much the study of the microscope retrograded at the close of the eighteenth century. Nearly all the authors of the second half of that century produced only very meagre observations, when compared with the researches of those that preceded them, although they also, like Leuwenhoek and Hooke, constructed their own instruments.

The puerility of Ledermüller is conspicuous in the descriptions he gives of the scales of fishes (*Amusemens microscopiques*, Nuremb. 1764, pl. 29, 38, 59, 93). "I believe, he says, that whoever would take the trouble to examine and make drawings of the scales of all sorts of small fishes, might form a cabinet of *coquillages* as pretty as curious." Of their structure he had formed no idea; as a proof of this we may mention the scale of the eel, which, according to him, is covered "with an infinity of large and small plates of an oval figure." We shall spare our readers the history of paste, and the considerations of this author on the "authentic proof of the infinite wisdom of the Almighty."

Fontana (*Lui le Venin de la Vipère*, Florence, 1781, vol. ii. p. 254) communicated a few microscopical observations on the gluten of eels, which are possessed of no value. He believes it to be formed of small bladders filled with very minute spherical corpuscules. When dried on glass, these become more irregular, and a corpuscule appears in their interior. Finally, on breaking these bladders, a great quantity of minute corpuscules were seen to issue.

In a memoir *On the scales of many species of fishes which are commonly believed to be destitute of these parts* (Journ. de Physiq. 1787, t. xxxi. p. 12), Broussonet merely describes the form of some scales as seen by the naked eye, or by the microscope. In that article we observe the following statement: "The peasantry of many northern countries were acquainted with the scales of the eel long before Leuwenhoek, since they collected them with care, in order to mix them with the white-wash which they put on the walls of their houses, as they thereby acquired a very agreeable lustre, particularly when the sun shone on them."

Heusinger (*Histologie*, vol. i. *Histographie*, Eisenach, 1822)

has seen, as well as Reaumur, the silvery matter composed of small angular corpuscules, among which are likewise found others of a black, shining, and angular appearance.

Kuntzmann *(Verhandlungen der Gesellschaft naturforschender Freunde in Berlin;* Berlin 1824, p. 269; 1829, p. 369) has published only the first part of a very extensive work on the scales of fishes, which, however, aims at no other object than the description of their different forms. The author cannot succeed in his researches, because, from his ignorance of the true organization of scales, he must misinterpret their structure, and even misunderstand their forms; but he is the first, as far as we know, who has advanced the opinion that their forms may afford distinctive characters in the different families. It is to be regretted that he has not followed out this idea, and that he has preferred to classify scales according to their form, a classification which is not satisfactory. Kuntzmann has made his observations on scales in a dry state; he believes, with Reaumur, that the growth takes place not only at the edges but throughout the whole extent of the scale. The following are the different classes of scales according to this author:—*a*, membranous; these present no distinct concentric lines *(Gadus lota)*; *b*, semi-membranous; the posterior part as in the preceding, the anterior marked with lines *(Clupea harengus)*; *c*, simple, which present only concentric lines, without longitudinal ones *(Salmo salar)*; *d*, with a regular design, for example, *Murena anguilla*; *e*, with four distinct fields, for instance, *Cyprinus carpio*; *f*, bristly *(Scorpaena)*; the author thinks that the bristles are placed on the membrane which envelopes the scale; they may be made to fall off by maceration; *g*, spiny *(Perca lucioperca)*; the spines are a true continuation of the scale, and maceration does not make them fall off. The two latter classes are divided in like manner into fields

We shall afterwards see what errors attach to this classification; thus the concentric lines exist in the first class in the form of cells, and it is only their transparency that prevented M. Kuntzmann perceiving them, although he adds that he saw irregular ones on the dried scales. Neither does any difference exist, as the author imagines, between the scales of

the two last classes. At the same time, this memoir is of more value than all the preceding works on the subject.

Ehrenberg (*Ann. de Poggendorf*, vol. xxviii. Leipsic 1833, p. 470) has lately described the crystals of silvery matter formerly observed by Reaumur. He likewise gives an analysis, made by Henry Rose, of a small quantity of this substance. It is soluble in diluted nitric acid; the solution is not affected by ammonia, and only very slightly so by the addition of oxalic acid. The results of these experiments shew,—that this matter evaporates by heat, without residuum; that it contains no lime; that it is soluble in the acids, as well as in alcohol and the alkalis (accompanied with ebullition in the latter).

We shall afterwards give an account of our own experiments on the subject of these crystals, made under the microscope.

M. Agassiz, in recent times, has strongly drawn the attention of philosophers to the form of scales, by assuming them as the basis of his classification. After having said a few words, in the work quoted below, on the structure of the skin, he speaks first of the position of scales, and then of their form.

M. Agassiz explains the different forms of imbrication, without entering into details on the disposition of the skin, and continues thus: "It results from this that the position of the scales varies much; however, we can usually distinguish pretty regular series, which enable us to determine their position with accuracy, particularly in the case of imbricated scales. The series are disposed obliquely from the fore part backwards, from the middle of the back to the middle of the belly; these series may be called *dorso-ventral*. It is necessary further to distinguish the *superior and inferior demi-series*, and I would call those which extend from the lateral line to the back *medio-dorsal*, and distinguish the *anterior and posterior medio-dorsal*, according as we wish to indicate those which are directed from before backwards, or those which are inclined from behind forwards. The same thing should be done with the series below the lateral line, which I call *medio-ventral*, such as extend from the lateral line backwards or downwards, being the *posterior medio-ventral*, and those directed forwards *anterior medio-ventral*.

"Scales," continues M. Agassiz, "are contained in the mu-

cous cavities or in small bags formed by the chorion, to which, however, they do not adhere by means of vessels." We may observe that this point by no means appears to us to be settled; we shall afterwards bring forward observations which contradict the idea. "They are formed of corneous or calcareous plates or leaves, superimposed on each other, and which are secreted at the surface of the chorion; these leaves successively attach themselves to the lower surface of the preceding ones, to which they become soldered by layers of indurated mucus." This is Leuwenhoek's idea, only he called these leaves *scales*. "In order to form an accurate notion of this development, it must be first examined in genera of fishes in which the scales appear to present these dispositions in the most simple state, for example, the eel, blennies, cobitis, and leuciscus. . . . It is easy to convince oneself that the concentric lines of the anterior edge and those of the posterior edge are continuations of each other." Nothing can be more opposed to M. Agassiz's opinion than the scales mentioned, the concentric lines of which are nothing more than insulated cells.

"After macerating scales for some time in water, they can be easily divided into a great number of plates or leaflets more or less thick and of different sizes, but all retaining the shape of the scale. These leaflets are disposed above each other in such a manner, that the smallest occupy the centre of the scale and form its exterior part, while the largest, with their margin projecting beyond the preceding, are soldered successively to their lower surface. We thus see evidently that the concentric lines visible on the surface of scales are simply the edges of the leaflets composing them." The fact related by M. Agassiz can refer only to the leaflets of the lower bed, which are separated by maceration; his conclusion, therefore, is founded on a false interpretation, which, in other respects, without the aid of the microscope, cannot be correct. All the modifications observed in the form and nature of the surface in scales, arise from the forms of the augmenting leaflets, and the manner in which they are placed above each other. Layers of enamel are deposited on the outside of some scales (as among the Ganoides)."

With regard to the longitudinal canals, M. Agassiz calls them furrows. "There are grooves at the margin of their outer surface, which correspond to each other in the different leaflets, and multiply during the growth of the scale." In describing the different external forms which the contour of scales presents to the naked eye, M. Agassiz likewise mentions the lobes, and continues thus: "When these lobes are hardened in the form of small teeth or very sharp serratures, and are only found in the last leaflet (the preceding successively disappearing as they become blunter), scales are then produced having a simple serrated edge; but when they are found on many consecutive leaflets, the edge of the scale is covered with numerous rows of points, and it is then very rough to the touch."

We shall afterwards see that our explanation of these points differs materially from that given by M. Agassiz; that we find them to be distinctly organized formations, composed of an envelope and a dentiform body, which presents roots, different surfaces, many degrees of development according to position, and different forms in different families of fishes.

Lastly, M. Agassiz establishes the principal divisions of the class of fishes according to the form of the scales. "I believe that I have found," he says, "in the differences presented by scales, a means of tracing more exactly the natural affinities of all kinds of fishes. It cannot at least be disputed that the animals of this class possess, in their scaly integuments, a character peculiar to themselves, and not found to exist in any other class. The following are the orders and the names of the principal families: 1st Order: The Placoides.—Thus named on account of the irregularity of the solid parts of their integuments; these consist of masses of enamel, often of considerable dimensions, at other times reduced to small points, such as the rings of the rays and the different shagreened surfaces of rays and sharks. 2d Order: The Ganoides.—The character common to all these is the angular form of the scales, which are composed of two substances, namely, corneous or osseous leaflets placed one above another, and covered with a thick layer of enamel. M. Agassiz includes many families both recent and fossil in this order, such as the Sclerodermes, Gymnodon-

tes, Lophobranches, Goniodontes, Silures, and Sturgeons. 3d Order: The Ctenoides.—The scales are formed of plates pectinated on their posterior edge; the pectinations of these numerous plates superimposed on each other so that the margin of the under one projects beyond that above it, rendering these scales rough to the touch. This structure is particularly remarkable among the Chenodontes and Pleuronectes. Here likewise are arranged the Percoides, Polyacanthes, Scienoides, Sparoides, Scorpionoïdes, and the Aulostomes. We shall afterwards have an opportunity of learning that these combs are very far from being simple plates, so arranged that the under ones always project beyond the upper: we shall see that they are true teeth, all the development of which admit of being traced. This structure must needs have escaped M. Agassiz's observation, in consequence of the insufficiency of the means he employed. For the same reason, this distinguished observer has not discovered the form of the teeth in many of the families he has placed in the following order. 4th Order: The Cycloides.—The families belonging to this order have scales formed of simple plates, with a smooth border; a structure which does not prevent their surface being frequently ornamented with various designs, imprinted at once on all the scales on their outer part, which is not covered. Here we must place the Labroides, Muges, and Atherines, Scomberoides, Gadoides, Gobioides, Murenoides, Lucloides, Salmones, Clupeæ, and Cyprini. We shall subsequently see that this order contains families whose scales present the most strongly marked differences. Assuredly we cannot assimilate the scales of the Cyprini to those of the Gobioides, nor arrange the Muges with the Atherines; the Muges present distinct teeth, although it may be true that the scales are not rough to the touch. In the family of the Cyprini there is room for sub-divisions. In this case also, a simple magnifying glass produced quite an insufficient enlargement of the objects for such investigations. We shall revert to this interesting point, which has acquired great importance since M. Agassiz's inquiries regarding the form of scales.

In finishing the history of researches into the structure of scales, we shall only remark, that nearly all authors have

shared in the ideas of Leuwenhoek; and that it was impossible for them to comprehend truly either the form or organization of scales, from not having employed the most powerful means of investigation, that is to say the microscope, which has procured more complete results for us.*

Chap. III.—*Scales considered as affording Characters for Classification.*

The most important question in the study of natural history, is the determination of natural characters, that is to say, of characters which bring together the inherent properties of animals,—which are proper to them in their natural state,—and which can be studied either by observing an entire body, or only a few detached parts, and that always without the assistance of other sciences. Now, what is most important in these researches, is precisely to determine characters in intimate relation with the nature of the animal, its organization, the anatomy and physiology of its body, which are, in short, essential, and undergoing no change with the accidents of habitation, food, &c.

It is the just determination and appreciation of these characters which can furnish us with true and precise notions of species, genera, and families, and enable us to avoid the errors committed by those who imagine they have found, in the most insignificant details, sufficient characters to constitute a new species. Have we not thus seen botanists make two different species of the same flower, as it happened to grow on the sides or in the bottom of a ditch, and might, from this cause, more or less differ in the brilliancy of its flowers? Do we not daily see ichthyologists and zoologists creating new species according to changes of colour, size, &c.,—changes which are altogether accidental, and depend entirely on the influences of climate, food, &c. But, apart from the small value attaching

* The second chapter of Dr Mandl's paper is devoted to the explanation of the structure of scales. This we have been obliged, in the mean while, to omit, as it requires a greater number of illustrative figures to render it fully understood, than we can at present find room for. The omission, however, is less to be regretted, as it in no way affects the interest or value of the two other chapters, each of which is complete in itself.—Editor.

to such characters as these, there is still another point most commonly overlooked by naturalists, on which we wish to say a few words.

Any property whatever, which constitutes a natural character, may vary in different degrees, and thus constitute in its successive changes a continuous series. Relations unite all the members of this series; no one is separated from another by a real difference, and each member of this series may be replaced by another of the same series, without changing the nature of the being. Thus, for example, white and red colours, with all their variations, may be found in roses without distinction, and yet no new species be created. All the shades between white and red, form in this case, then, a natural series, the members of which may be substituted for each other without changing the nature of the rose.

Professor Mohs has made a very successful application of these principles to mineralogy, or, to speak more properly, these ideas of natural characters and the series they constitute, owe their origin to this distinguished philosopher. The creation of the systems of crystallization, such, for example, as the tessular, rhomboidal, pyramidal systems, &c., gave birth to as many natural series, containing a multitude of members, all of which may be substituted for each other, but which are in themselves essentially separated, and cannot be confounded. Thus it is of little importance, for the determination of a species, whether the mineral be crystallized under the form of a cube, or any other derived from that; but never can it present, for example, a pyramidal form, for there is no transition between a cube and a pyramid.

It is only characters, then, which distinguish series, and which admit of no transition, that can authorize us to establish distinctions between beings, and create new species. Whenever there is a passage between the characters of a new individual and those of one previously known, we cannot make a real division. Unhappily the neglect, or perhaps the ignorance, of these principles, has led many naturalists to create a multitude of species founded on distinctions quite futile, and (if I may be pardoned the expression) altogether foolish, producing an incumbrance of new species sufficient to cause disgust for the study of natural history.

There is only one way to remedy this abuse, that is to study true natural characters, to observe attentively the series they constitute, and to make a rigorous application of them in the classification of animals. Scales appear to us to offer one of these natural characters for the classification of fishes. The intimate connection subsisting between the tegumentary appendages and the organization of the animal, of which we have already spoken in the former chapter, affords a very strong argument in favour of our opinion. We do not wish to rest our case on the reason that, because fishes can live when deprived of their fins, but not when deprived of their scales, it therefore follows that scales have a greater physiological value than fins. Such an argument would be too imperfect and inconclusive.

The physiological importance of scales has but little to do with our present purpose; we only want to know, in this case, whether they can afford a distinctive character between different individuals. Now, scales preserve the same form, not only in the same individual, but in all the individuals of the same species; they are essentially different in individuals of another family; they constitute different series of form, series which are very distinct in themselves, but the members of which offer all the degrees of transition which unite them to each other. Scales may therefore serve as a natural character in the description and classification of fishes.

The merit of having first called the attention of naturalists to scales is due to M. Agassiz; but being deprived of the aid of the microscope, not only the organization of the scale remained concealed from him, but he inevitably also confounded scales of the most different forms. It is thus that M. Agassiz states,* that, in the family of the Cyprini, "all the body is covered with scales formed by a pretty considerable number of plates with the edges smooth and entire; grooves or furrows, more or less numerous, extend from the centre of growth to the edge of the scales," &c., and he arranges the Cyprini in the family of the Cycloides; but these furrows

* Mémoirs de la Société des Sciences Naturelles de Neufchâtel t. i. p. 34.

are canals—these entire and smooth edges of the plates are nothing else than lines resulting from the fusion of the cells, lines which are repeated on all the scales. We thus see that, from the organization not being well known, a characteristic description of scales could not be given, and some were confounded with others which yet offered true marks distinctive of families. We find an example of this in the memoir of M. Agassiz above cited.

This distinguished savant supposes (l. c. p. 48), that the relations which connect the Muges and Atherines with the Cyprini have entirely escaped Cuvier, on account of the too great importance which this celebrated naturalist attached to the presence or absence of spiny rays on the back. M. Agassiz had therefore to seek some character common to all these fishes, that he might be able to bring them together, and he says that he discovered this character in their scales, which are all composed of plates of growth with entire edges, and which he calls Cycloides.

Now nothing strictly can be more different than the scales of the Muges, Atherines, and Cyprini. The difference is so great, that it alone determined us to place the Atherines in a separate family, a measure which had previously been hinted at by Cuvier, and which he would not have hesitated definitively to adopt, had he known the particular characters presented by their scales. We have seen with pleasure that our opinion is supported by Professor Nordmann, who makes an entirely distinct family of the Atherines, as well as of *Mullus*.

We can no longer affirm, with M. Agassiz, that Fitzinger has done wrong in separating Cyprinus from Cobitis. We shall not here discuss the reasons which determined the latter to effect this separation, but it is certain that the microscopic inspection of the scales justifies it completely, and that this difference alone authorizes a distinction to be made between these two genera.

We shall not say more in this place in confirmation of our opinion. The examples adduced already demonstrate sufficiently, that the detailed study of scales by the aid of the microscope, can alone reveal their forms. Of this we find a

convincing proof in the incomplete results M. Agassiz obtained by studying them with the naked eye. His vast knowledge, and the care with which he conducted his researches, could not compensate for the insufficiency of his means of observation.

Another question here arises: how far can the scales afford marks of distinction between species, genera, and families? It will be readily understood, that the detailed and continued study of a great number of well preserved individuals, can alone decide this. It may even be found that the same form reappears in different families, and that the other characters must concur in effecting a classification, in the same manner as the same form of crystallization recurs in minerals altogether different. Hitherto, we have found the forms very distinct and characteristic in each family. If we have not been able to prosecute our researches to the distinction of genera and species, it is for want of a sufficient number of individuals; at the same time we do not believe that we will require to renounce the attempt altogether. Our ulterior observations will throw light on this subject. Meanwhile we have been able to establish differences between families, whose scales M. Agassiz considered identical, as has been proved in the preceding pages.

Report on the Manufacture of Tea, and on the Extent and Produce of the Tea-Plantations in Assam. By C. A. Bruce, Superintendent of Tea-Culture.* With a map of the Tea Districts.

I submit this report on our Assam Tea with much diffidence, on account of the troubles in which this frontier has

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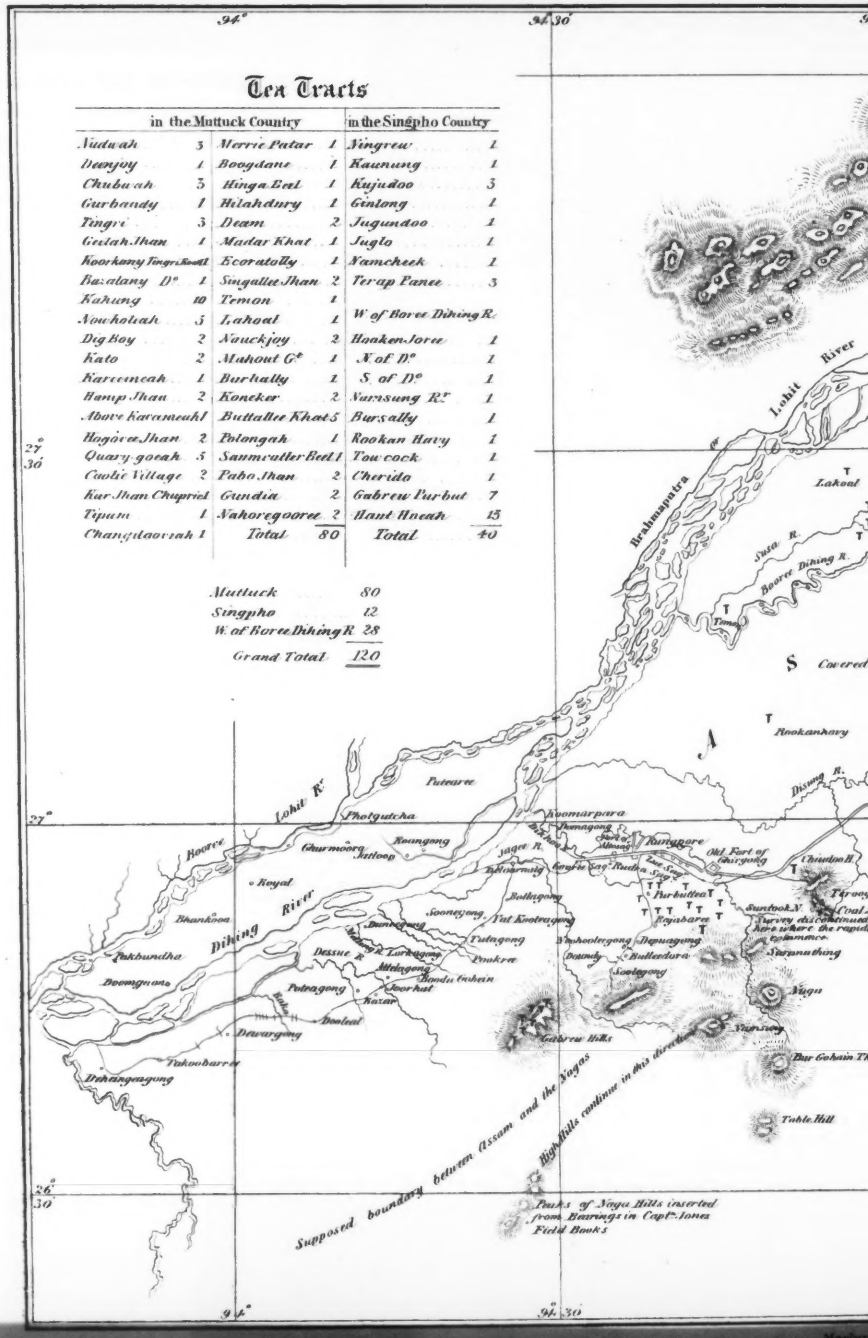
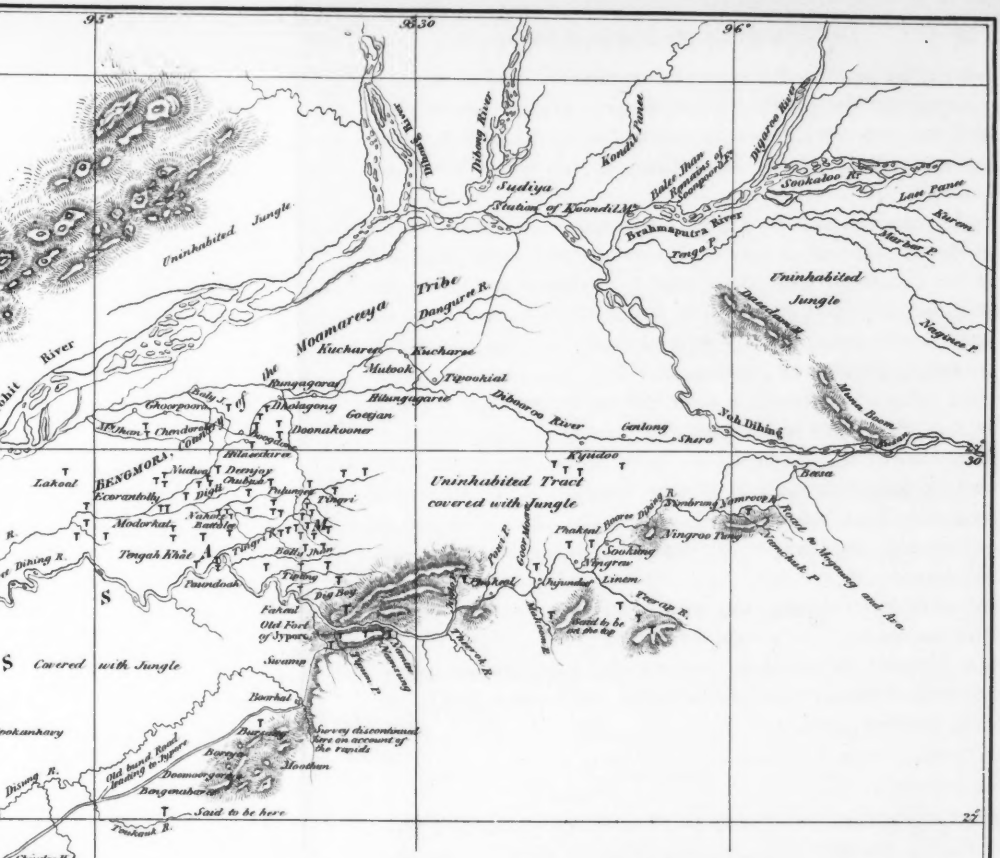
* Presented by the Tea Committee appointed by Government (consisting of James Patch, Esq., T. W. Grant, Esq., and C. K. Robison, Esq., Dr Nathaniel Wallich, Raja Rada Kauth Deva, and Babao Rom Comul Sea), and read at the meeting of the Society on Wednesday 14th August 1839. Printed in Calcutta 21st September 1839, and dispatched same day by overland mail, and communicated to us by Sir John Robison, K. H., Secretary of the Royal Society of Edinburgh, &c. &c.


94°
94° 30
Tea Tracts
in the Muttuck Country
Nuduah 3
Deunjoy 1
Chubuah 3
Gurhandy 1
Tingri 3
Gutlah Than 1
Koorkamy Tingri Small 1
Baxalany Dº 1
Kuhung 10
Nowhotiah 3
Dig Boy 2
Kato 2
Kareencah 1
Hanip Than 2
Above Karamcuhl 1
Hagover Than 2
Quary gocah 5
Cuolie Village 2
Kur Than Chupriel 1
Tipum 1
Changdaoriah 1
Merrie Patar 1
Boogdane 1
Hinga Eeel 1
Helahdury 1
Deam 2
Madar Khat 1
Ecoratolly 1
Singallee Than 2
Temon 1
Lahaal 1
Nauckjoy 2
Mahout Gº 1
Burhally 1
Koneker 2
Buttallee Khat 3
Polongah 1
Sanmvutter Beel 1
Pabo Than 2
Gundia 2
Nahoregooree 2
Total 80
in the Singpho Country
Yingrew 1
Kaunung 1
Kujudoo 3
Gintong 1
Jugundoo 1
Jugto 1
Namcheek 1
Terap Panee 3
W. of Boree Dihing R.
Hoaken Joree 1
N. of Dº 1
S. of Dº 1
Nurasung Rº 1
Bursally 1
Rookan Havy 1
Tow cock 1
Cherido 1
Gabrew Purbut 7
Hant Hoeah 15
Total 40
Muttuck 80
Singpho 12
W. of Boree Dihing R. 28
Grand Total 120
27° 30
27°
26° 30
Lohit River
Brahmapootra
Lakoal
Sisa R.
Boree Dihing R.
Tenga
Rookanhavy
Dissing R.
Puteare
Lohit R.
Phetgutcha
Boomarpara
Boore
Dihing River
Ghurmoora
Kaungong
Jagee R.
Royal
Bhankoon
Takhundha
Doomgoora
Dessue R.
Potragong
Dewargong
Dholial
Dihangmagong
Jaipore
Old Fort of Jaipore
Purbuttea
Royabara
Suntook N.
Survey discontinued here where the rapid commences
Saranathing
Nagas
Gabrew Hills
Table Hill
Supposed boundary between Assam and the Nagas
High Hills continue in this direction
Peaks of Naga Hills inserted from Bearings in Captn. Jones Field Books
94°
94° 30



MAP
OF
MUTTOCK, SINGPHO,
and
The Country West of the Borre Dibing River.

Shewing all the Tea Tracts that have hitherto been discovered by

Mr. C. A. Bruce, Superintendent of

TEA CULTURE

To

The Honble: East India Company

IN ASSAM.

Sypore 1st June 1839.

XUM

been unfortunately involved. I have had something more than tea to occupy my mind, and have consequently not been able to commit all my thoughts to paper at one time; this I hope will account for the rambling manner in which I have treated the subject. Such as my report is, I trust it will be found acceptable, as throwing some new light on a subject of no little importance to British India, and the British public generally. In drawing out this report, it gives me much pleasure to say, that our information and knowledge respecting tea and tea-tracts are far more extensive than when I last wrote on this subject;—the number of tracts now known amounting to 120, some of them very extensive, both on the hills and in the plains. A reference to the accompanying map will shew that a sufficiency of seeds and seedlings might be collected from these tracts in the course of a few years to plant off the whole of Assam; and I feel convinced, from my different journeys over the country, that but a very small portion of the localities are as yet known.

Last year, in going over one of the hills behind *Jaipore*, about 300 feet high, I came upon a tea-tract, which must have been two or three miles in length,—in fact I did not see the end of it; the trees were in most parts as thick as they could grow, and the tea seeds (smaller than what I had seen before) fine and fresh, literally covered the ground: this was in the middle of November, and the trees had abundance of fruit and flower on them. One of the largest trees I found to be two cubits in circumference, and full forty cubits in height. At the foot of the hill I found another tract, and, had time permitted me to explore those parts, there is no doubt but I should have found many of the Naga Hills covered with tea. I have since been informed of two more tracts near this. In going along the foot of the hills to the westward, I was informed that there was tea at *Teweack*, or near it: this information came too late, for I had passed it, just a little to the east of the Dacca River, at a place called *Cheriedoo*, a small hill projecting out more than the rest on the plain to the northward, with the ruins of a brick temple on it; here I found tea, and no doubt, if there had been time to examine, I should have found many more tracts. I crossed the

Dacca River at the old fort of *Ghergong*, and walked towards the hills, and almost immediately came upon tea. The place is called *Hauthoweah*. Here I remained a couple of days going about the country, and came upon no fewer than thirteen tracts. A Dewaniah who assisted me to hunt out these tracts, and who was well acquainted with the leaf, as he had been in the habit of drinking tea during his residence with the Singphoes, informed me that he had seen a large tract of tea-plants on the Naga mountains, a day's journey west of *Chiridoo*. I have no reason to doubt the veracity of this man; he offered to point out the place to me, or any of my men, if they would accompany him; but as the country belonged to Raja Poorunda Sing, I could not examine it. I feel convinced the whole of the country is full of tea.

Again, in going further to the south-west, just before I came to *Gabrew* hill, I found the small hills adjoining it, to the eastward, covered with tea-plants. The flowers of the tea on these hills are of a pleasant delicate fragrance, unlike the smell of our other tea-plants; but the leaves and fruit appear the same. This would be a delightful place for the manufacture of tea, as the country is well populated, has abundance of grain, and labour is cheap. There is a small stream called the *Jhangy* river, at a distance of two hours' walk; it is navigable, I am informed, all the year round for small canoes, which could carry down the tea, and the place is only one and a half day's journey from *Jorehaut*, the capital of Upper Assam. South-west of *Gabrew Purbut* (about two days' journey) there is a village at the foot of the hill, inhabited by a race called Norahs; they are Shans, I believe, as they came from the eastward, where tea abounds. I had long conversations with them, and the oldest man of the village, who was also the head of it, informed me, that when his father was a young man, he had emigrated with many others, and settled at *Tipum* opposite *Jaipore*, on account of the constant disturbances at *Munkum;* that they brought the tea-plant with them, and planted it on the *Tipum* hill, where it exists to this day; and that when he was about sixteen years of age, he was obliged to leave *Tipum*, on account of the wars and disturbances at that place, and take shelter at the village where he now re-

sides. This man said he was now eighty years of age, and that his father died a very old man. How true this story is, I cannot say, and do not see what good it would do the old man to fabricate it. This was the only man I met with in my journeys about the country who could give any account of the tea-plant, with the exception of an Ahum, who declared to me that it was Sooka, or the first Kacharry raja of Assam, who brought the tea-plant from *Munkum;* he said it was written in his *Putty*, or history. The *Ahum-Putty* I have never been able to get hold of; but this I know, that the information about the tea-plant pointed out by the old Norah man, as being on the *Tipum* hill, is true; for I have cleared the tract where it grew thickest, about 300 yards by 300, running from the foot of the hill to the top. The old man told me his father cut the plant down every third year, that he might get the young leaves.

To the west of *Gabrew* I did not find any tea; but to the westward of the *Dhunseeree* river I found a species, though not the same as that we use. If the people on the west side of the *Dhunseeree* river were acquainted with the true leaf, I think tea would be found. I planted it all along the route I went, which may lead to its eventual discovery; but people should be sent to search for the plant who are really acquainted with it. I think a vast quantity of tea would be brought to light if this were done. A reference to the map will shew how our tracts are distributed all over the country. But how much tea they would all produce if fully worked, I will not pretend to say; but in the course of this subject I will mention such matters relative to the tracts and plants on them, that every one may make his own calculation. Until lately, we had only two Chinese black tea-makers. These men have twelve native assistants; each Chinaman, with six assistants, can only superintend one locality, and the tea-leaves from the various other tracts, widely separated, must be brought to these two places for manufacture. The consequence is, that an additional number of labourers must always be employed to bring the leaves from so great a distance. The leaves suffer when brought in large quantities from a distance, as they soon begin to ferment, and the labour of only preparing them so

far in process, that they may not spoil by the morning, is excessive. The men have often to work until very late to accomplish this. When labour falls so very heavy, and on so very few, it cannot be expected that it can be equally well executed, as if more had been employed. The leaves last gathered are also much larger than they ought to be, for want of being collected and manufactured earlier; consequently the tea is inferior in quality. I mention this to shew the inconvenience and expense of having so few tea-makers.

The samples of black tea made by the twelve assistants having been approved of by the Tea Committee in Calcutta, it was my intention to have distributed the men among the different tracts; but the late disturbances on our frontier have prevented this arrangement, and I have been obliged to employ ten men in Assam (two others having gone to Calcutta in charge of tea) at the tract called *Kahung*, which is becoming a very extensive and important tea locality, so many others being near it, which can all be thrown into one. When we have a sufficient number of manufacturers, so that we can afford to have some at each tract or garden, as they have in China, then we may hope to compete with that nation in cheapness of produce; nay, we might, and ought, to undersell them; for if each tract or garden had its own tea-maker and labourers, the collecting of the leaves would not perhaps occupy more than twelve days in each crop; after which the men might be discharged, or profitably employed on the tea-grounds. But now, for the want of a sufficient number of labourers and tea-makers, there is a constant gathering of leaves throughout the month; and, as I said before, those gathered last can only make inferior teas; besides, the great loss by the leaves getting too old, and thereby unfit for being made into any tea; and all this entirely for want of hands to pluck the leaves. It is true we have gained twelve black tea-makers this year, in addition to the last; and twelve more native assistants have been appointed, who may be available next year to manufacture tea independently, as they were learning the art all last year. We have also had an addition to our establishment of two Chinese green tea manufacturers, and twelve native assistants have been placed under them to learn; but

what are these compared to the vast quantity of tea, or the ground the tea-plants cover, or might be made to cover, in three years, but a drop of water in the ocean? We must go on at a much faster pace in the two great essentials—tea manufacturers and labourers—in order to have them available at each garden, when the leaves come into season.

If I were asked, when will this tea experiment be in a sufficient state of forwardness, so as to be transferable to speculators? I would answer, when a sufficient number of native tea-manufacturers have been taught to prepare both the black and the green sort; and that under one hundred available tea-manufacturers, it would not be worth while for private speculators to take up the scheme on a large scale; on a small one it would be a different thing. In the course of two or three years we ought to have that number. Labourers must be introduced in the first instance to give a tone to the Assam opium-eaters; but the great fear is that these latter would corrupt the new comers. If the cultivation of tea were encouraged, and the poppy put a stop to in Assam, the Assamese would make a splendid set of tea-manufacturers and tea-cultivators.

In giving a statement of the number of tea-tracts, when I say that *Tingri*, or any other tract, is so long and so broad, it must be understood that space to that extent only has been cleared, being found to contain all the plants which grew thickly together; as it was not thought worth while, at the commencement of these experiments, to go to the expense of clearing any more of the forest for the sake of a few straggling plants. If these straggling plants were followed up, they would, in all probability, be found gradually becoming more numerous, until you found yourself in another tract, as thick and as numerous as the one you left; and if the straggling plants of this new tract were traced, they would by degrees disappear, until not one was seen. But if you only proceeded on through the jungles, it is ten to one that you would come upon a solitary tea-plant, a little further on you would meet with another; until you gradually found yourself in another new tract, as full of plants as the one you had left, growing absolutely so thick as to impede each other's growth.

Thus I am convinced one might go on for miles from one tract into another. All my tea-tracts about *Tingri* and *Kahung* are formed in this manner, with only a patch of jungle between them, which is not greater than what could be conveniently filled up by thinning those parts that have too many plants. At *Kahung* I have lately knocked three tracts into one, and I shall most probably have to continue doing the same until one tract shall be made of what now consists of a dozen. I have never seen the end of *Juggundoo's* tea-tract, nor yet *Kujudoo's* or *Ningrew's*. I feel confident that the two former run over the hills and join, or nearly join, some of our tracts in the *Muttack* country. Nor have I seen the end of *Kahung* tract, all about that part of the country being one vast succession of tea, from *Rungagurra* on the *Debrew* to *Jaipore* on the *Buri Dehing*. It may be seen, on inspecting the map, how thickly the tea localities are scattered;—those that are known, and they are but a small portion compared to those that are unknown. There is the *Namsong* tract on the *Naga* hills, the largest that has yet been seen, and the extent of which is not ascertained. The tracts on the *Gubind* hills are unknown; and this is likewise the case with *Haut Holah* and *Cheridoo;* so that there is a large field for improvement throughout, to say nothing of the *Singpho* tracts, which may be found to be one unbounded link to *Hookum;* and who knows but it crosses the Irrawaddy to China? Many tea-tracts I know have been cut down in ignorance by the natives, to make room for the rice field, for fire-wood, and fences. Many of these tracts have sprung up again, more vigorous than before. Witness that at *Ningrew*, where the natives say that every thing was cut down, and the land planted with rice, except on the high ground.

With respect to the tea-plant being most productive on high or low ground, I cannot well say, as all our tracts are on the plains; but from what little I have seen of the hill tracts, I should suppose they were not more productive. In China, the hill tracts produce the *best* teas, and they may do the same here. Almost all my tracts on the plains are nearly on the same level, I should think. *Nudwa*, perhaps, is a little higher than *Tingri*, and *Tingri* a little higher than *Kahung*, but I

believe they are equally productive; although, if I leaned towards any side with my limited experience, I should say that the low land, such as at *Kahung*, which is not so low as ever to be inundated by the strongest rise in the river, is the best. The plants seem to love and court moisture, not from stagnant pools, but running streams. The *Kahung* tracts have the water in and around them; they are all in heavy tree-jungles, which makes it very expensive to clear them. An extent of 300 by 300 will cost from 200 to 300 rupees; *i. e.* according to the manner in which the miserable opium-smoking Assamese work. This alone ought to point out the utility of introducing a superior race of labourers, who would not only work themselves, but encourage their women and children to do the same;—in plucking and sorting leaves, they might be profitably turned to account for both parties. This I have not been able to instil into the heads of the Assamese, who will not permit their women to come into the tea-gardens. Indeed, unless more labourers can be furnished, a large amount of tea must not be looked for at present. Last season it was with the greatest difficulty that I could get a sufficient number of hands to gather the leaves. The plucking of the leaves may appear to many a very easy and light employment, but there are not a few of our coolies who would much rather be employed on any other job; the standing in one position so many hours occasions swellings in the legs, as our plants are not like those of China only three feet high, but double that size, so that one must stand upright to gather the leaves. The Chinese pluck theirs squatting down. We lie under a great disadvantage in not having regular men to pluck the leaves; those that have been taught to do so, can pluck twice as many as those that have not, and we can seldom get hold of the same men two seasons running. I am of opinion that our trees will become of a smaller and more convenient size after a few years' cultivation; because, trimming of the plants and taking all the young leaves as soon as they appear, month after month, and year after year, and the plants being deprived of the rich soil they had been living on from time unknown, must soon tell upon them. Transplanting also helps to stunt and shorten the growth of these plants. The Chinese declared to me,

that the China plants now at *Deenjoy* would never have attained to half the perfection they now have, under ten years in their own country.

I may here observe, that the sun has a material effect on the leaves; for as soon as the trees that shade the plants are removed, the leaf, from a fine deep green, begins to turn into a yellowish colour, which it retains for some months, and then again gradually changes to a healthy green, but now becomes thicker, and the plant throws out far more numerous leaves than when in the shade. The more the leaves are plucked, the greater number of them are produced; if the leaves of the first crop were not gathered, you might look in vain for the leaves of the second crop. The tea made from the leaves in the shade, is not near so good as that from leaves exposed to the sun; the leaves of plants in the sun, are much earlier in season than of those in the shade; the leaves from the shady tract give out a more watery liquid when rolled, and those from the sunny a more glutinous substance. When the leaves of either are rolled on a sunny day, they emit less of this liquid than on a rainy day. This juice decreases as the season advances. The plants in the sun have flowers and fruit much earlier than those in the shade, and are far more numerous; they have flowers and seeds in July, and fruit in November. Numerous plants are to be seen, that, by some accident, either cold or rain, have lost all their flowers, and commence throwing out fresh flower-buds more abundantly than ever. Thus it is not unfrequent to see some plants in flower so late as March (some of the China plants were in flower in April), bearing at once the old and the new seeds, flower-buds, and full-blown flowers,—all at one and the same time. The rain also greatly affects the leaves, for some sorts of tea cannot be made in a rainy day; for instance the *Powchong* and *Mingehew*. The leaves for these ought to be collected about 10 A. M. on a sunny morning, when the dew has evaporated. The *Powchong* can only be manufactured from the leaves of the first crop; but the *Mingehew*, although it requires the same care in making as the other, can yet be made from any crop, provided it is made on a sunny morning. The Chinese dislike gathering leaves on a rainy day for any description of tea, and never

will do so, unless necessity requires it. Some pretend to distinguish the tea made on a rainy and on a sunny day, much in the same manner as they can distinguish the shady from the sunny teas—by their inferiority. If the large leaves for the black tea were collected on a rainy day, about seven seers, or fourteen pounds, of green leaves would be required to make one seer, or two pounds, of tea; but if collected on a sunny day, about four seers, or eight pounds, of green leaves, would make one seer, or two pounds, of tea;—so the Chinamen say. I tried the experiment, and found it to be correct. Our season for tea-making generally commences about the middle of March; the second crop in the middle of May; the third crop about the first of July; but the time varies according to the rains setting in sooner or later. As the manufacture of the *Sychee* and *Mingehew* black teas has never been described, I will here attempt to give some idea how it is performed.

Sychee Black Tea.—The leaves of this are the *Souchong* and *Powchong*. After they have been gathered and dried in the sun in the usual way (see my former account of black tea), they are beaten and put away four different times; they are then put into baskets, pressed down, and a cloth put over them. When the leaves become of a brownish colour by the heat, they throw out and have a peculiar smell, and are then ready for the pan, the bottom of which is made red-hot. This pan is fixed in masonry breast high, and in a sloping position, forming an angle of forty degrees. Thus the pan being placed on an inclined plane, the leaves when tossed about in it cannot escape behind, or on the sides, as it is built high up, but fall out near the edge close to the manufacturer, and always into his hands, so as to be swept out easily. When the bottom of this pan has been made red-hot by a wood fire, the operator puts a cloth to his mouth, to prevent inhaling any of the hot vapour. A man on the left of him stands ready with a basket of prepared leaves; one or two men stand on his right with dollahs, or shallow baskets, to receive the leaves from the pan, and another keeps lifting the hot leaves thrown out of the pan into the dollah, that they may quickly cool. At a given signal from the Chinaman, the person with the

Thus I am convinced one might go on for miles from one tract into another. All my tea-tracts about *Tingri* and *Kahung* are formed in this manner, with only a patch of jungle between them, which is not greater than what could be conveniently filled up by thinning those parts that have too many plants. At *Kahung* I have lately knocked three tracts into one, and I shall most probably have to continue doing the same until one tract shall be made of what now consists of a dozen. I have never seen the end of *Juggundoo's* tea-tract, nor yet *Kujudoo's* or *Ningrew's*. I feel confident that the two former run over the hills and join, or nearly join, some of our tracts in the *Muttack* country. Nor have I seen the end of *Kahung* tract, all about that part of the country being one vast succession of tea, from *Rungagurra* on the *Debrew* to *Jaipore* on the *Buri Dehing*. It may be seen, on inspecting the map, how thickly the tea localities are scattered;—those that are known, and they are but a small portion compared to those that are unknown. There is the *Namsong* tract on the *Naga* hills, the largest that has yet been seen, and the extent of which is not ascertained. The tracts on the *Gubind* hills are unknown; and this is likewise the case with *Haut Holah* and *Cheridoo;* so that there is a large field for improvement throughout, to say nothing of the *Singpho* tracts, which may be found to be one unbounded link to *Hookum;* and who knows but it crosses the Irrawaddy to China? Many tea-tracts I know have been cut down in ignorance by the natives, to make room for the rice field, for fire-wood, and fences. Many of these tracts have sprung up again, more vigorous than before. Witness that at *Ningrew*, where the natives say that every thing was cut down, and the land planted with rice, except on the high ground.

With respect to the tea-plant being most productive on high or low ground, I cannot well say, as all our tracts are on the plains; but from what little I have seen of the hill tracts, I should suppose they were not more productive. In China, the hill tracts produce the *best* teas, and they may do the same here. Almost all my tracts on the plains are nearly on the same level, I should think. *Nudwa*, perhaps, is a little higher than *Tingri*, and *Tingri* a little higher than *Kahung*, but I

believe they are equally productive; although, if I leaned towards any side with my limited experience, I should say that the low land, such as at *Kahung*, which is not so low as ever to be inundated by the strongest rise in the river, is the best. The plants seem to love and court moisture, not from stagnant pools, but running streams. The *Kahung* tracts have the water in and around them; they are all in heavy tree-jungles, which makes it very expensive to clear them. An extent of 300 by 300 will cost from 200 to 300 rupees; *i. e.* according to the manner in which the miserable opium-smoking Assamese work. This alone ought to point out the utility of introducing a superior race of labourers, who would not only work themselves, but encourage their women and children to do the same;—in plucking and sorting leaves, they might be profitably turned to account for both parties. This I have not been able to instil into the heads of the Assamese, who will not permit their women to come into the tea-gardens. Indeed, unless more labourers can be furnished, a large amount of tea must not be looked for at present. Last season it was with the greatest difficulty that I could get a sufficient number of hands to gather the leaves. The plucking of the leaves may appear to many a very easy and light employment, but there are not a few of our coolies who would much rather be employed on any other job; the standing in one position so many hours occasions swellings in the legs, as our plants are not like those of China only three feet high, but double that size, so that one must stand upright to gather the leaves. The Chinese pluck theirs squatting down. We lie under a great disadvantage in not having regular men to pluck the leaves; those that have been taught to do so, can pluck twice as many as those that have not, and we can seldom get hold of the same men two seasons running. I am of opinion that our trees will become of a smaller and more convenient size after a few years' cultivation; because, trimming of the plants and taking all the young leaves as soon as they appear, month after month, and year after year, and the plants being deprived of the rich soil they had been living on from time unknown, must soon tell upon them. Transplanting also helps to stunt and shorten the growth of these plants. The Chinese declared to me,

that the China plants now at *Deenjoy* would never have attained to half the perfection they now have, under ten years in their own country.

I may here observe, that the sun has a material effect on the leaves; for as soon as the trees that shade the plants are removed, the leaf, from a fine deep green, begins to turn into a yellowish colour, which it retains for some months, and then again gradually changes to a healthy green, but now becomes thicker, and the plant throws out far more numerous leaves than when in the shade. The more the leaves are plucked, the greater number of them are produced; if the leaves of the first crop were not gathered, you might look in vain for the leaves of the second crop. The tea made from the leaves in the shade, is not near so good as that from leaves exposed to the sun; the leaves of plants in the sun, are much earlier in season than of those in the shade; the leaves from the shady tract give out a more watery liquid when rolled, and those from the sunny a more glutinous substance. When the leaves of either are rolled on a sunny day, they emit less of this liquid than on a rainy day. This juice decreases as the season advances. The plants in the sun have flowers and fruit much earlier than those in the shade, and are far more numerous; they have flowers and seeds in July, and fruit in November. Numerous plants are to be seen, that, by some accident, either cold or rain, have lost all their flowers, and commence throwing out fresh flower-buds more abundantly than ever. Thus it is not unfrequent to see some plants in flower so late as March (some of the China plants were in flower in April), bearing at once the old and the new seeds, flower-buds, and full-blown flowers,—all at one and the same time. The rain also greatly affects the leaves, for some sorts of tea cannot be made in a rainy day; for instance the *Powchong* and *Mingehew*. The leaves for these ought to be collected about 10 A. M. on a sunny morning, when the dew has evaporated. The *Powchong* can only be manufactured from the leaves of the first crop; but the *Mingehew*, although it requires the same care in making as the other, can yet be made from any crop, provided it is made on a sunny morning. The Chinese dislike gathering leaves on a rainy day for any description of tea, and never

will do so, unless necessity requires it. Some pretend to distinguish the tea made on a rainy and on a sunny day, much in the same manner as they can distinguish the shady from the sunny teas—by their inferiority. If the large leaves for the black tea were collected on a rainy day, about seven seers, or fourteen pounds, of green leaves would be required to make one seer, or two pounds, of tea; but if collected on a sunny day, about four seers, or eight pounds, of green leaves, would make one seer, or two pounds, of tea;—so the Chinamen say. I tried the experiment, and found it to be correct. Our season for tea-making generally commences about the middle of March; the second crop in the middle of May; the third crop about the first of July; but the time varies according to the rains setting in sooner or later. As the manufacture of the *Sychee* and *Mingehew* black teas has never been described, I will here attempt to give some idea how it is performed.

Sychee Black Tea.—The leaves of this are the *Souchong* and *Powchong*. After they have been gathered and dried in the sun in the usual way (see my former account of black tea), they are beaten and put away four different times; they are then put into baskets, pressed down, and a cloth put over them. When the leaves become of a brownish colour by the heat, they throw out and have a peculiar smell, and are then ready for the pan, the bottom of which is made red-hot. This pan is fixed in masonry breast high, and in a sloping position, forming an angle of forty degrees. Thus the pan being placed on an inclined plane, the leaves when tossed about in it cannot escape behind, or on the sides, as it is built high up, but fall out near the edge close to the manufacturer, and always into his hands, so as to be swept out easily. When the bottom of this pan has been made red-hot by a wood fire, the operator puts a cloth to his mouth, to prevent inhaling any of the hot vapour. A man on the left of him stands ready with a basket of prepared leaves; one or two men stand on his right with dollahs, or shallow baskets, to receive the leaves from the pan, and another keeps lifting the hot leaves thrown out of the pan into the dollah, that they may quickly cool. At a given signal from the Chinaman, the person with the

basket of prepared leaves seizes a handful and dashes it as quick as thought, into the red-hot pan. The Chinaman tosses and turns the crackling leaves in the pan for half a minute, then draws them all out by seizing a few leaves in each hand, using them by way of a brush, not one being left behind. They are all caught by the man with the dollah or basket, who, with his disengaged hand, continues lifting the leaves and letting them fall again, that they may quickly cool. Should a leaf be left behind in the pan by any accident, the cloth that is held ready in the mouth is applied to brush it out; but all this is done as quick as lightning. The man that holds the basket of leaves watches the process sharply; for no sooner is the last leaf out of the pan, than he dashes in another handful, so that, to an observer at a little distance, it appears as if one man was dashing the leaves in, and the other as fast dashing them out again—so quickly and dexterously is this managed. As soon as one basket has received about four handsful of the hot leaves from the pan, it is removed, and another basket placed to receive the leaves; and so on, until all is finished. A roaring wood fire is kept up under the pan, to keep the bottom red-hot, as the succession of fresh leaves tends greatly to cool the pan, which ought always to be scrubbed and washed out after the process is over. In China these pans are made of cast-iron, and if great care is not taken, they will crack in the cooling; to prevent which, one man keeps tapping the inside of the edge of the pan briskly with a wet broom, used in the cleaning of the vessel, while another pours cold water in gently; thus it cools in a few seconds, and is ready for another batch of tea. The leaves are rolled and tatched the same as the other teas, and put into the drying basket for about ten minutes. When a little dry, people are employed to work and press the leaves in the hands in small quantities, of about one and a half to two rupees' weight at a time, for about half a minute; they are then put into small square pieces of paper and rolled up; after this they are put into the drying basket, and permitted to dry slowly over a gentle fire for some hours, until the whole is thoroughly dry. This tea is not sold in the China market; it is used principally as offerings to the priests, or kept for high days and holidays. It is

said to be a very fine tea, and there is not one man in a hundred who can make it properly. The *Powchong* tea is made in the same way as the *Sychee*, with this exception, that it is not formed into balls.

Mingehew Black Tea.—The leaves (*Powchong*) are plucked and dried in the sun, and are then beaten and dried in the shade for half an hour; this is done three successive times, and the leaves are very much shaken by a circular motion given to them in a sieve, so as to keep them rolling and tumbling about in the centre of it. This treatment continues until they are very soft; they are then allowed to remain for a short time; the contents of the first sieve are then placed in the centre of a close-worked bamboo basket, with a narrow edge, and the leaves are divided into four equal parts. The contents of the second sieve are placed in another bamboo basket like the former, and this basket is placed on the top of the first, and so on, piling one basket upon another, until all is finished;—there may be about two pounds of leaves in each basket. The red-hot pan is used the same as in *Sychee*, only now the men cast in one division of the leaves into the basket, and this is tumbled and tossed about in the red-hot pan like a plaything for about thirty seconds, and then swept out; another division is cast in, and so on, until all the prepared baskets have been emptied. The contents of each basket are still kept separate, by placing the leaves, when they come out of the pan, in separate baskets. The whole is a brisk and lively scene, and quite methodical, every one knowing his station, and the part he has to perform. The baskets are then arranged on shelves to air; the contents are afterwards tatched the same as our black teas, and fired in the drying baskets, but with this difference, that each division is placed on paper and dried. When it is half dry (the same as our teas), it is put away for the night, and the next morning it is picked, and put into the drying baskets over gentle deadened fires, and gradually dried there; it is then packed hot. This tea is a difficult sort to make.

Shung Paho Black Tea.—Pluck the young (*Paho*) leaf that has not yet blown or expanded, and has the down on it, and the next one that has blown, with a part of the stalk; put it

into the sun for half an hour, then into the shade; tatch over a gentle fire, and in tatching roll the leaves occasionally in the pan, and spread them all round the sides of the same; again roll them until they begin to have a withered and soft appearance, then spread them on large sieves, and put them in the shade to air for the night; next morning pick, and then fire them well. Some tea-makers do not keep them all night, but manufacture and pack the tea the same day. This tea is valued in China, as it is very scarce; but the Chinamen acknowledge that it is not a good sort. They prefer the teas the leaves of which are come to maturity.

The China black tea-plants which were brought into *Muttack* in 1837, amounted in all to 1609—healthy and sickly. A few of the latter died, but the remainder are healthy, and flourish as well as if they had been reared in China. The leaves of these plants were plucked in the beginning of March, and weighed sixteen seers, or thirty-two pounds. Many of the plants were then in flower, and had small seeds. They are about three feet high, and were loaded with fruit last year, but the greater part of it decayed when it had come to maturity, as was the case with the Assam tea-seeds, and almost every seed of these wilds, in the past year. The seeds should, I think, be plucked from the plant when thought ripe, and not be permitted to drop or fall to the ground. I collected about twenty-four pounds of the China seeds, and sowed some on the little hill of *Tipum*, in my tea-garden, and some in the nursery-ground at *Jaipore*, above three thousand of which have come up, are looking beautiful, and doing very well. I have since found out that all the China seedlings on *Tipum* hill have been destroyed by some insect.

The Assam and China seedlings are near each other; the latter have a much darker appearance. I have made but few nurseries, or raised plants from seed, as abundance of young plants can be procured, of any age or size, from our tea-tracts. There may be about 6000 young seedlings at *Chubwa;* at *Deenjoy* about 2000; at *Tingri* a few; and some at *Paundooah*. In June and July 1837, 17,000 young plants were brought from *Muttack*, and planted at a place called *Toongroong Patar*, amongst the thick tree jungles of *Sadiya*.

In March of the same year, six or eight thousand were

brought from *Muttack*, and planted in different thick jungles at *Sadiya;* many of these died in consequence of the buffaloes constantly breaking in amongst them; the rest are doing well, but I am afraid will be killed from the above cause; and, now that I have removed to *Jaipore*, they are too far off for my personal superintendence.

In 1838, 52,000 young tea-plants were brought from the *Nemsong Naga* hill tracts, about ten miles from *Jaipore;* a great portion of these have been lately sent to Calcutta, to be forwarded to Madras; should they thrive there, it is my opinion that they will never attain any height, at least not like ours, but be dwarfish like the China plants. *Deenjoy*, *Chubwa*, *Tingri*, and *Geela-Jhan* tracts have been filled up or enlarged with plants from the jungle tracts. In transplanting from one sunny tract to another, when done in the rains, very few, if any, die; if the plants be removed from a deep shade to a sunny tract, the risk is greater, but still, if there be plenty of rain, few only will die. If from a deep shade to a piece of ground not a tea-tract, and exposed to the sun—for instance, from the *Naga* hills to *Jaipore*—if there be plenty of rain, and the soil congenial, as it is at this place, few will die; if shaded by a few trees, less will perish; if taken from shade, and planted in shade, and the soil uncongenial, but there is plenty of rain, the greater portion will live;—witness *Toongroong Patar* at *Sadiya*. If the plants are brought from deep shade, and planted in the sun, in uncongenial soil, let them have ever so much rain, not one in fifty will be alive the third year;—witness 30,000 brought to *Sadiya*. I believe the tea-plant to be so hardy that it would almost live in any soil, provided it were planted in deep shade when taken to it. There should be plenty of water near the roots, but the plants should always be above inundation. As soon as it has taken root, which it will soon do, the shade may be removed, and there will be no fear of the plant dying.

The advantage of getting plants from the jungle tracts is, that you can get them of any age or size; nothing more is necessary than to send a few coolies early in March, just as the rains commence, and have the plants of the size required removed to your own garden; and if they are of a moderate

size, you may gather a small crop of tea from them the next year. As these plants are very slender, it would be best to plant four or five close together to form a fine bush. If the plants are raised from seed, you may expect a small crop of tea the third year, but they do not come to maturity under six years. It is said they live to the age of forty or fifty years. The Chinese way of digging a hole, and putting in a handful or two of seed, does not succeed so well in this country, as putting two or three seeds on small ridges of earth, and covering them over, which I have found to answer better.

In clearing a new tea-tract, if the jungle trees are very large and numerous, it would be as well to make a clean sweep of the whole, by cutting them and the tea-plants all down together; for it would be impossible to get rid of so much wood without the help of fire. The tea-plants, if allowed to remain, would be of little use after they had been crushed and broken by the fall of the large trees, and dried up by the fire; but admitting that they could escape all this, the leaves of trees from twelve to twenty feet high could not be reached, and, if they could, they would be almost useless for tea-manufacture, as it is the young leaves from young trees that produce the best teas. But if all were cut down and set fire to, we should have a fine clear tract at once, at the least expense, and might expect to have a pretty good crop of tea one year after the cutting, or at furthest, the second year; for it is astonishing with what vigour the plant shoots up after the fire has been applied. And we gain by this process; for, from every old stock or stump cut down, ten or twelve more vigorous shoots spring up, so that, in the place of a single plant, you have now a fine tea-bush. I think, from what I have seen of these plants, that, if cut down every third year, they would yield far superior teas; neither am I singular in this opinion, the green tea Chinamen having told me that they cut down their plants every ninth year, which may be reckoned equivalent to our third year, taking into consideration the size of our trees and the richness of our soil. Our trees, or plants, are certainly more than four or five times the size of theirs, and must consequently yield so many times more produce; theirs is the dwarf, ours the giant tea. The

size of the leaf matters nothing, in my opinion, provided it is young and tender, even their diminutive leaf, if one day too old, is good for nothing.

As the green tea Chinamen have just commenced operations, I will try to give some account of this most interesting process. All leaves up to the size of the *Souchong* are taken for the green tea. About three pounds of the fresh leaves, immediately they are brought in, are cast into a hot pan (sometimes they are kept overnight when abundance have been brought in, and we have not been able to work all up); they are then rolled and tossed about in the pan, until they become too hot for the hand. Two slips of bamboo, each about a foot long, split at one end so as to form six prongs, are now used to tumble and toss the leaves about, by running the sticks down the sides of the pan, and turning the leaves up first with the right hand, then with the left, and this as fast as possible; which keeps the leaves rolling about in the pan without being burnt: this lasts about three minutes; the leaves will then admit of being rolled and pressed without breaking. They are now taken from the pan and rolled in dollahs, much the same as the black tea, for about three minutes, in which process a great quantity of the juice is extracted, if they be fresh leaves; but if they have been kept over night, very little juice can be expressed from them in the morning, on account of its having evaporated. The Chinamen say, this does not matter, as it makes no difference in the tea. The leaves are then pressed hard between both hands, and turned round and pressed again and again, until they have taken the shape of a small pyramid. They are now placed in bamboo baskets or dollahs with a narrow edge, and the dollahs or bamboo framework (see fig. 2 of my former account of black tea) where they are exposed to the sun for two or three minutes, after which these pyramids of tea are gently opened, and thinly spread on the dollahs to dry. When the tea has become a little dry (which will be the case in five or ten minutes if the sun be hot), it is again rolled, and then placed in the sun as before; this is done three successive times. But should the weather be rainy, and there is no hope of its clearing, all this drying is done over the fire in a small drying basket, the same as

with black tea. The green tea makers have as great an aversion to drying their tea over the fire as the black tea makers. The third time it has been rolled and dried, there is very little moisture left in the tea; it is now put into a hot pan, and gently turned over and over, and opened out occasionally, until all has become well heated; it is then tossed out into a basket, and while hot put into a very strong bag, previously prepared for it, about four feet long, and four spans in circumference. Into this bag the tea is pressed with great force with the hands and feet; from fourteen to twenty pounds being put in at one time, and forced into as small a compass as possible. With his left hand the man firmly closes the mouth of the bag immediately above the leaves, while, with the right hand, he pommels and beats the bag, every now and then giving it a turn; thus he beats and turns and works at it, tightening it by every turn with one hand, and holding on with the other, until he has squeezed the leaves into as small a compass as possible at the end of the bag. He now makes it fast by turns of the cloth where he held on, so that it may not open; and then draws the cloth of the bag over the ball of leaves, thus doubling the bag, the mouth of which is twisted and made fast. The man then stands up, holding on by a post or some such thing, and works this ball of leaves under his feet, at the same time alternately pressing with all his weight, first with one foot and then the other, turning the ball over and over, and occasionally opening the bag to tighten it more firmly. When he has made it almost as hard as a stone, he secures the mouth well and puts the bag away for that day. Next morning it is opened out and the leaves gently separated and placed on dollahs, then fired and dried until they are crisp, the same as the black tea, after which they are packed in boxes or baskets. In China the baskets are made of double bamboo, with leaves between. The tea may then remain on the spot for two or three months, or be sent to any other place to receive the final process. This first part of the green tea process is so simple, that the natives of this country readily pick it up in a month or two. The second process now commences by opening the boxes or baskets, and exposing the tea on large shallow bamboo baskets or dollahs (see former account, fig. 1)

until it has become soft enough to roll; it is then put into cast-iron pans, set in brick fire-places, the same as described in making the *Sychee* black tea. The pan is made very hot by a wood-fire, and seven pounds of the leaves are thrown into it and rubbed against the pan, with the right hand until tired, and then with the left, so as not to make the process fatiguing. The pan being placed on an inclined plane, the leaves always come tumbling back towards and near the operator, as he pushes them up from him, moving his hand backwards and forwards and pressing on the leaves with some force with the palms, keeping the ends of the fingers up to prevent their coming in contact with the hot pan. After one hour's good rubbing, the leaves are taken out and thrown into a large coarse bamboo-sieve, from this into a finer one, and again still finer one, until three sorts of tea have been separated. The first, or largest sort, is put into the funnel of the winnowing machine, which has three divisions of small traps below, to let the tea out. A man turns the wheel with his right hand, and with the left regulates the quantity of tea that shall fall through the wooden funnel above, by a wooden slide at the bottom of it. The tea being thrown from the sieves into the funnel, the man turns the crank of the wheel, and moves the slide of the funnel gradually, so as to let the tea fall through gently, and in small quantities. The blast from the fan blows the smaller particles of tea to the end of the machine, where it is intercepted by a circular moveable board placed there. The dust and smaller particles are blown against this board and fall out at an opening at the bottom into a basket placed there to receive it. The next highest tea is blown nearly to the end of the machine, and falls down through a trough on the side into a basket; this tea is called *Young Hyson*. The next being a little heavier, is not blown quite so far; it falls through the same trough, which has a division in the middle; this of course is near the centre of the machine. A basket is placed beneath to receive the tea, which is called *Hyson*. The next, which is still heavier, falls very near to the end of the fan, this is called *Gunpowder* Tea; it is in small balls. The heaviest tea falls still closer to the fan, and is called *Big Gunpowder;* it is twice or three times the size of *Gunpowder* tea, and com-

posed of several young leaves that adhere firmly together. This sort is afterwards put into a box and cut with a sharp iron instrument, then sifted and put among the *Gunpowder*, which it now resembles. The different sorts of tea are now put into shallow bamboo baskets, and men, women, and children are employed to pick out the sticks and bad leaves; this is a most tedious process, as the greatest care is taken not to leave the slightest particle of any thing but good tea. But to assist and quicken this tiresome process, beautiful bamboo sieves, very little inferior to our wire ones, and of various sizes, are employed. The different teas are thrown into sieves of different sizes, from large gunpowder to dust tea; they are shaken and tossed, and thrown from one person to another in quick succession, making the scene very animating; in this way a great portion of the stalks are got rid of. After the tea has been well sifted and picked, it is again put into the hot pans and rubbed and rolled as before, for about one hour; it is then put into shallow bamboo baskets, and once more examined, to separate the different teas that may still remain intermixed, and again put into the hot pan. Now a mixture of sulphate of lime and indigo, very finely pulverized and sifted through fine muslin, in the proportion of three of the former to one of the latter, is added; to a pan of tea containing about seven pounds, about half a tea-spoonful of this mixture is put and rubbed and rolled along with the tea in the pan for about an hour, as before described. The tea is then taken hot from the pan and packed firmly in boxes, both hands and feet being used to press it down. The above mixture is not put to the tea to improve its flavour, but merely to give it a uniform colour and appearance, as without it some of the tea would be light and some dark. The indigo gives it the colour, and the sulphate of lime fixes it. The Chinese call the former *Youngtin*, the latter *Acco*. Large gunpowder tea they call *Tychen;* little gunpowder, *Cheocheu;* hyson, *Chingcha;* young hyson, *Uchin;* skin tea, or old leaves in small bits, *Poocha;* the fine dust or powder tea, *Chamoot*.

The leaves of the green tea are not plucked the same as the black, although the tree or plant is one and the same, which has been proved beyond a shadow of doubt; for I am

now plucking leaves for both green and black from the same tract and from the same plants; the difference lies in the manufacture, and nothing else. The green tea-gatherers are accommodated with a small basket, each having a strap passed round the neck, so as to let the basket hang on the breast. With one hand the man holds the branch, and with the other plucks the leaf, one at a time, taking as high as the *Souchong* leaf; a little bit of the lower end of the leaf is left for the young leaf to shoot up close to it; not a bit of stalk must be gathered. This is a very slow and tedious way of gathering. The black tea-maker plucks the leaves with great rapidity with both hands, using only the forefinger and thumb, and collects them in the hollow of the hand; when his hand is full he throws the leaves into a basket under the shade of the tree; and so quickly does he ply his hands that the eye of a learner cannot follow them, nor see the proper kind of leaf to be plucked; all that he sees, is the Chinaman's hands going right and left, his hands fast filling, and the leaves disappearing. Our coolies, like the green tea Chinamen, hold the branch with one hand, and deliberately pluck off the leaf required, then the next, and so on, by which process much time is lost, and a greater number of hands are wanted. Not having a regular set of pluckers is a very great drawback to us; for the men whom we teach this year we see nothing of the next; thus every year we have to instruct fresh men. This difficulty will be removed when we get regular people attached to the tea-plantations; or when the natives of these parts become more fixed and settled in their habitations, and do not move off by whole villages from one place to another, as they have of late years been doing; and when the aversion they have throughout Assam to taking service for payment has been overcome. They seem to hold this as mean and servile; preferring to cultivate a small patch of ground which barely yields a subsistence. I can perceive, however, that there is a gradual change taking place in the minds of the labouring class of people, or coolies; for occasionally some good able-bodied men come forward for employment. The generality of those that have hitherto offered themselves, has been from the very poorest and the most worthless in the country. In

the cold season, when the men have nothing to sow or reap, two or three hundred can be collected; but as soon as the rains set in, all but those who have not bounds, or are not involved in debt, go off to their cultivations, at the very time when our tea-operations commence. As long as things continue in this state, the price of tea will be high; but if this drawback were removed, there is nothing to prevent our underselling the Chinese, except the experience of a few more years.

But let us return to our teas, and take a comparative view of the qualities of the black and green teas, which may nearly be as follows: *Paho* black tea leaf would make green tea, some gunpowder, and some young hyson. *Pouchong*, although classed as a second black tea, on account of the high price it fetches in the market, is a third-rate leaf, for it is rather larger than the *Souchong*. Some of it would make young hyson, and some skin tea. *Souchong* would make hyson and young hyson. *Toychong* would make skin tea.—I will here mention the different kinds of black teas, to make the matter more clear to those who take an interest in the subject. *Thowung-Paho* (the *Sung fa* is the same leaf as this) is the downy little leaf not expanded, and the one next to it that has just unfolded a little. This tea when made appears full of small white leaves, which are the little downy leaves just mentioned. *Twazee-Paho* is from the second crop, and nearly the same kind of tea, only a little older; the leaf next the small downy one (being a little more expanded) and the small leaf below this, are taken, making three in all; this has also numerous white leaves, but not so many as the former. *Souchong* is the next largest leaf; this is well grown, but embraces all the leaves above it. When the upper leaves have grown out of season for *Thowung-Paho* and *Twazee-Paho*, they are all plucked for the *Souchong* from the third and fourth of the upper leaves. From *Souchong* leaves the *Minchong* and *Sychee* teas are made in the first crop, and no other. *Pouchong* is the next largest leaf; it is a little older and larger than the *Souchong*. From this leaf the *Sychee* and *Minchong* teas can be made in the first crop only. The *Pouchong* is never made in the second crop, on account of its not having a good flavour; many of

the *Souchong* leaves are mixed up in this tea. The *Toychong* leaves are those that are rejected from the *Souchong* and *Pouchong*, as being too large and not taking the roll. When the teas are picked, these leaves are put on one side. The Chinese often put them into a bag, and give them a twist, something in the green tea way, and then mix them up with the *Souchong* to add to the weight. This leaf (*Toychong*) becomes worse in the second and third crops;—it is a cheap tea and sold to the poor. All the black teas that are damaged have the flower of what the Chinese call *Qui fa*, another called *Son fa*, mixed up with them. One pound of the flowers is put to each box of damaged tea. After the teas have been well tatched and mixed up with other sorts, these leaves give them a pleasant fragrance. The *Son fa* plant is about two feet high, and kept in flower-pots; it is propagated from the roots. The *Qui fa* plant is from three to four feet high; one pound of the flowers is put to a box of tea. The plant was seen in the Botanical Gardens at Calcutta by our Chinese interpreter. The flowers of this plant are considered finer than those of the *Son fa*. I annex a rough drawing of each of them, as given to me by the interpreter; the dots in the drawings are intended for small flowers.*

The black tea-makers appear to me to be very arbitrary in their mode of manufacture; sometimes they will take the leaves of the *Thowung-Paho*, or perhaps *Twazee-Paho;* but if it has been raining, or there is any want of coolies to pluck the leaves quickly, or from any other cause, they will let the leaves grow a few days longer, and turn all into *Souchong;* which, it must be remembered, takes all the small leaves above it. If it is the first crop, the *Souchong* and *Pouchong* leaves may all be turned into *Souchong* tea; but even if it is the

* These two sketches are not deemed sufficiently instructive to be added here. One of them is entitled *Qui fa*, which is the name of the Olea fragrans, or Sweet-scented Olive, the flowers of which are said to be used for perfuming teas. But it is more like the Aglaia odorata, a very different plant, which is also supposed to be applied in China for a similar purpose. This last, however, is called *Tsjiulang* by the Chinese according to Rumph, and *Sam yeip lan* according to Roxburgh. The other sketch, entitled *Lan fa*, seems to be intended for a liliaceous, or, at any rate, an endogenous plant. I am unable to offer any conjecture about it.—N. W.

second crop, when the *Pouchong* leaves ought not to be gathered, they are nevertheless plucked and mixed up with the *Souchong* leaves. Almost all our black and all the green teas have just been made from one garden. When the green tea makers complained that the leaves were beginning to get too large for them—that is, they were fast growing out of *Souchong* and running into *Pouchong*—the black tea makers took up the manufacture; plucked all the leaves, and made excellent *Pouchong;* so that between the two, there is not a leaf lost. When the black tea-makers have a garden to themselves they are cruel pluckers, for they almost strip the tree of leaves for the *Souchong*, and are not at all nice in the plucking; the third and even the fourth leaf on a tender twig is nipped off in the twinkling of an eye; they then look about for more young leaves, and away go the *Pouchong*, and *Toychong* too, which is the largest leaf of all. But the green tea-men pluck quietly, one by one, down to *Souchong*. The black tea men separate all their teas into first, second, third, and fourth crop; but the green tea manufacturers make no distinction; they prepare all the tea they can, throughout the season, box or basket it up, and when the season is over, they set off for Canton with their produce; at least all those who do not wish to sell their tea on the spot. The different merchants go in quest of it there. It now indiscriminately undergoes the second process; that is, the different crops are all mixed up together. No old leaves can be mixed in the green, as in the black teas; for the long rolling in the pan crushes them, and the fan blows them away, so that only the young leaves are left.

We shall now take a comparative view of the number of men required by the black and the green tea makers for one pair of pans.

For the black tea-makers there will be required,

To tatch, ...	2 men
... roll, ...	4 ...
... attend to the fire, ...	1 ...
.. dry, ...	1 ...
... beat and put in the sun, . . .	2 ...
Total number of men, .	10

To keep these men fully at work, from twenty-five to thirty coolies will be required to pluck leaves, and they will turn out about two boxes of tea per day (weighing one maund, or 80 pounds), if the weather be fine and sunny; but scarcely half that quantity if it be rainy, on account of the coolies not plucking so much on a rainy, as they would on a fair sunny day. As the people of the country become acquainted with the gathering and manufacturing, three boxes, of forty pounds each, may be expected in fine weather, adding perhaps a few men to the number of coolies.

A pair of pans for the green tea-makers would require, during the first process,

To tatch, ...	2 men.
... receive the tea from the pans, . .	1 ...
... roll, ...	8 ...
... attend to the fire, ...	1 ...
... put the leaves in the sun and turn them,	4 ...
Total number of men, .	16

Thirty coolies would be required to keep these men in full play, and they would turn out two boxes of twenty-three seers, or forty-six pounds each, per day; in all ninety-two pounds of tea. If the weather be rainy, of course the produce is much less, as the gatherers then do only half work. Thus the difference between the black and green is, that the former requires six manufacturers less; and that when the black tea is finished, boxed, and ready for exportation, the green has only undergone the first process, and is but half finished, although it is ready for exportation to any appointed place, to receive the final and troublesome, as well as most expensive part of the process. Nevertheless the first part of the green tea preparation is easily learnt by the natives of this place, in about two or three months. In speaking of the trouble and expense attending the second process of the green tea-making, I beg to observe that it appears to me, from what little I have seen of it, that machinery might easily be brought to bear; and as Assam is about to become a great tea country, it behoves us to look to this. The tea half made, as above described, I am informed by the green tea Chinamen now with me, is put either into boxes or baskets, with bamboo leaves between; it

has to make in this state a long journey by land and water, and then to go one or two months in a boat by sea, before it reaches Canton, where it is laid aside for one or two months more, before it undergoes the second process; making in all about five months from the time it was first prepared. All that is required is to keep it dry. Now if all this be true, which I have no doubt it is, I see no reason why we could not send it to England, and have it made up there. I rather see every thing in favour of such a plan, and nothing against it. After a year's instruction under Chinamen, it might be left to the ingenuity of Englishmen to roll, sift, and clean the tea by machinery, and, in fact, reduce the price of the green tea nearly one-half, and thus enable the poor to drink good unadulterated green tea, by throwing the indigo and sulphate of lime overboard. At all events, the experiment is worthy of a fair trial, and the first step towards it would be to manufacture the tea at Calcutta; or perhaps it would be better to let the China green tea-makers go direct to England along with it, and have it manufactured there at once.

Now for a word about the lead-canister maker, who is a very important man in our establishment; for without him we could not pack our teas.—On two tiles about an inch thick and sixteen inches square, is pasted, on one side, a sheet of very fine thick paper, said to have been made in Cochin-China, over this another sheet is pasted only at the edges. The paper must be very smooth, and without any kind of hole, knob, or blemish. To make it answer the purpose better, fine chalk is rubbed over it. The tiles thus prepared are laid one over the other, and moved backwards and forwards, to ascertain if they work smoothly. The lower tile rests on two pieces of wood, about four inches in thickness, and the exact length of the tile. The room where the sheets of lead are made must be very smooth and level, as the tiles are apt to break when there is any unequal pressure on them. In the corner of the room there is a sunken brick fire-place, the upper part of which rises just a little above the floor; into this fire-place is inserted one of the cast-iron pans used for making tea, and in one corner of the masonry is a vent hole, on which in general a tea-kettle

stands. The pan is heated by a wood-fire; an iron ladle with a handle, about six or eight inches long, answers the purpose of taking the lead out of the pan when required. The pan may hold about twenty pounds. There is also another ladle with a long handle, and holes at the bottom, to take the dross off. When lead for the sides of the boxes is required, the proportion of one maund of lead to five seers of tin is put into the pan. When well melted and freed from dross, the two tiles above mentioned are placed on the two pieces of wood, one piece being nearly under the centre, and the other at the edge of the lower tile; the upper tile is placed on the lower tile even and square, projecting perhaps a little backwards towards the operator. The tiles being thus placed near the melted lead, the Chinaman squats down on them, placing his heels near the edge, with his toes towards the centre; while with his left hand he lays hold of the corner tile, and with the right holds the short ladle, which he dips into the boiler, and takes out about half a ladleful of the molten lead, tipping up the upper tile with the left hand about three inches, at the same time assisting this operation by pressing on his heels and gently lifting his toes. The upper tile being thus raised, he dashes in the contents of the ladle between both, lets go with the left hand, and presses on with his toes, which brings the upper tile with some force to its former position over the lower one, and occasions the superfluous lead to gush out right and left and in front. The upper tile is then raised like the lid of a box, while the lower one rests on the piece of projecting wood underneath, and a fine thin sheet of lead, nearly the size of the tiles, is taken out, and thrown on one side; the upper tile is then gently lowered down, another ladle of hot lead dashed in, and so on in quick succession, about four sheets of lead being made in one minute. The lower tile projecting a little beyond the upper one, assists the man to lay the ladle on, and pour in the metal firmly and quickly. To vary the operation, the man sometimes stands up and places one foot on the upper tile, working with his heel and toes, the same as if both feet were on, and just as quickly. Many interruptions take place, such as examining the papers on the tiles, rubbing them with chalk, turning them

round, and reversing them. Sometimes half a split bamboo is placed in front and under the tiles, with a piece of paper on it, to receive the lead that falls down, so that it may not come in contact with the ground. This lead is every now and then taken up and put back into the boiler. A maund of lead may make about twelve or thirteen boxes, which will hold forty pounds. There are also two other tiles, about a cubit square, these are used for making the tops of the canisters, which are generally of tin only, but can also be made from the above mixture. It is necessary in making this sheet-lead, to hold the sheets up and examine them; for if not properly prepared, there are sometimes a number of very fine holes in them, which are not perceptible when lying on the ground or table. On this account the first twenty sheets of lead are thrown aside and rejected, even without any examination. When the tiles have become nice and warm, it is then the fine and even sheets, without holes, are obtained. Before a sheet-lead canister can be made, it is necessary to have a model-box made to fit into the wooden box, that is to hold the sheet-lead canister; on this box or shell the sheet-lead canister is made. It has a hole at the bottom to prevent any suction in putting it in, or drawing it out of the box or canister; and instead of a top it has a bar of wood across, by which it is drawn out. For soldering, tin, with the eighth or twelfth part of quicksilver, and some rosin, are used. The wood-part of some of the boxes is covered with paper pasted on and dried in the sun. To give the paper on the boxes a yellow colour, a mixture of paste, with pulverized and sifted saffron, is laid on and dried. The paper on the corners of the boxes is ornamented by means of a wooden block with flowers carved on it; on this bit of wood, very thin paper, cut to its size, is placed, and a mixture, consisting of pulverized saffron, indigo, and water, having a deep green colour, is laid singly on each bit of paper, with a brush made of cocoa-nut fibres. These slips of paper are put one above the other, twenty thick, or as long as the paper takes the impression of the carved wood below. When the corners of the boxes have been ornamented with this paper and dried, another mixture, about the proportion of four seers of oil to three seers of rosin, boiled together, is applied with a cocoa-

nut brush over all the boxes as a finish; after these are dry they are ready for the tea.

The following table will shew the size and produce of the tea-tracts now worked, and the probable amount of tea for this and the next season.

Names of Tea-tracts fully worked in 1838.	Length and breadth of Tea-tracts.	Number of Plants in each Tea-tract.	Average produce of single Tea-plants.	Produce in 1838.
No. 1 Tringri,	267 by 90	5000	4 Sa. Weight,	260 Srs.
No. 2 Tringri,	155 by 70	2340	3-12 Sa. Wt.,	160 ...
No. 1 Kahung,	480 by 210	1,36000	4 Sa. Weight,	680 ...
No. 1 Chubwa,	200 by 160	8200	4 Sa. Weight,	410 ...
Deenjoy,* .	223 by 171	8400	2 Sa. Weight.	210 ...
				1720 ...
From shady tracts, ...				390 ...
				2110 ...
The probable increase of the above tracts for 1839,				527 ...
Probable produce of 1839, . .				2637 Srs. (5274 lb.)

Names of the Tracts to be worked in 1840.	Length and breadth of Tea-tracts.	Number of Plants in each Tea-tract.	Probable produce of one Tea-plant.	Probable Produce in 1840.
No. 2 Kahung,	192 by 114	4720	3 Sa. Weight,	177
No. 3 Ditto,	215 by 70	3440	3 Sa. Weight,	129
No. 2 Chubwa,	160 by 70	2420	3 Sa. Weight,	90
Nowholea,	476 by 160	16480	3 Sa. Weight,	618
Tipun,†	344 by 331	24620	3 Sa. Weight,	922
Jugundoo,†	400 by 200	17300	3 Sa. Weight,	648
Ningrew,†	300 by 189	12260	3 Sa. Weight,	459
The probable produce of the above 7 tracts, . .				2943
And the probable produce of the other 5 tracts, .				2637
Probable produce of all the tracts in 1840, . .				5580 (11,160 lb.)

It should be borne in mind that this is a rough calculation, and I can only give the probable amount. Most of these plants are very young, or have been recently cut down; a few years hence the plants may yield twice the above quantity. The

* The plants are small in this tract, including China plants.

† The plants in these tracts, now small, will not yield a good crop for two years.

first table exhibits the absolute produce of 1838. Now let us suppose a new settler were to take land in these parts, what would be his expenses if he were only to cultivate tea, and had to clear forest land (in the vicinity of the tea) ten times the size of *Nowholea*, which is, say 400 by 200 yards, and which would cost him 200 rupees to clear. Ten such tracts would cover 800,000 square yards. Now, to cover this surface of ground with tea-plants, and the plants six feet apart each way, 355,555 plants would be required; but if two plants were to be placed together, as I would recommend, then 711,110 plants would be required. The cost would probably be at the rate of five annas for 300 plants; thus:

The clearing of 10 tracts, each 400 by 200 yards,	2000	0	0
711,110 tea-plants, at 5 annas for 300,	740	11	8
Planting the above,	474	0	0
Weeding each tract 3 times each year, at 30 rs. each tract,	900	0	0
5 tea-houses, at 50 rs. each,	250	0	0
200 hoes, at 1 rupee each,	200	0	0
100 axes at 1 rupee each,	100	0	0
100 daws at 1 rupee each,	100	0	0
Dollahs, challonis, &c. bamboo apparatus,	200	0	0
8 saws, at 5 rs. each,	40	0	0
Charcoal and firewood for baking the tea,	200	0	0
40 cast-iron pans, at 4 rupees each,	160	0	0
Paper for tea-boxes,	100	0	0
Chalk and indigo,	50	0	0
3 maunds of nails of sizes, at 10 rupees per maund,	30	0	0
2 elephants, at 150 rupees each,	300	0	0
2 elephant mahoots, at 6 rupees each per month,	144	0	0
2 elephant mates, at 4 rupees each per month,	96	0	0
Rice for 2 elephants,	96	0	0
Lead for 888 boxes, at 3 seers per box containing 20 seers, at 8 rupees per maund,	532	12	9
A Cooly sirdar at 10 rupees per month,	120	0	0
10 Duffadars, or overseers of coolies, at 3 rupees per month,	360	0	0
Coolies to collect leaves, 30 to each tract, 20 days to each crop; for 3 crops, or 60 days, at 3 rupees for each man per month,	1800	0	0
4 native carpenters, at 12 rupees ditto,	576	0	0
8 sawyers, at 4 rupees ditto,	384	0	0
2 native lead canister-makers, at 12 rupees ditto,	288	0	0
Coolies to bring in timber for sawyers,	150	0	0
5 Chinamen, at 30 rupees each per month,	1800	0	0
Carry forward,	12,191	8	5

	Brought forward,	12,191	8 5
120 native tea-makers, at 5 rupees each for 5 months or one season,		3000	0 0
Freight to Calcutta,		400	0 0
Ditto to England,		1000	0 0
	Total outlay for 10 tracts, Co's rupees,	16,591	8 5

Deduct charges that are not annual, viz.:—

Clearing of tracts,	2000	0	0	
Purchase of tea-plants,	740	0	0	
Planting ditto,	474	0	0	
Building tea-houses,	150	0	0	
Purchase of hoes,	200	0	0	
Do. axes,	100	0	0	
Do. daws,	100	0	0	
Do. saws,	40	0	0	
Do. bamboo apparatus,	200	0	0	
Do elephants,	300	0	0	
			4304	0 0
Total annual outlay on 10 tracts,		12,287	8 5	
Average produce of 355,555 tea-plants, at 4 sa. wt. each plant, is 444 mds., or 17,777 srs., or 35,554 lb., at 2s. or 1 rupee per pound, would be,		35,554	0 0	
Annual profit on 10 tracts, Co.'s rupees,		23,266	7 7	

Annual Outlay.	Co.'s rs.	Annual Profits.	Co.'s rs.
For 10 tracts,	12,287	On 10 tracts,	23,266
For 100 tracts,	1,22,870	On 100 tracts,	2,32,660
For 1000 tracts,	12,28,700	On 1000 tracts,	2326,600

N.B.—The deduction of 4304 rs. not being annual outlay, is not included in this calculation above 10 tracts.

	Tea Tract.	*Duffadars.*	*Takelah.*	*Coolies.*
Required for	1	1	10	30
—	10	10	100	300
—	100	100	1000	1000

It must be remembered that this calculation has been made on 355,555 plants, not on double that number, as I proposed, viz. to plant them in pairs, which would certainly, on the lowest calculation, increase the profits thirty per cent. It should be borne in mind also, that 4 sicca weight is not the full produce of each plant; when full grown, it will yield double that, or 8 sicca weight, and some even as high as 10 to 12 sicca weight. I have calculated at the rate of 4 sicca, which was absolutely

produced in 1838. The plant will, I should think, produce 25 per cent. this year, and go on increasing to what I have above mentioned. But then, on the other hand, the items which I have set down are not all that will be required to carry on this trade on an extensive scale. The superintendence, numerous additional artizans that will be required, and a thousand little wants which cannot be set down now, but which must necessarily arise from the nature of the cultivation and manufacture, will go far to diminish the profits and swell the outlay; but this of course will last but a few years, until the natives of the country have been taught to compete with Chinamen. It should also be remembered, that the calculation I have made on ten tracts is on a supposition that we have a sufficient number of native tea-makers and canister-makers, which will not be the case for two or three years to come. It is on this point alone that we are deficient, for the tea-plants and lands are before us. Yes, there is another very great drawback to the cultivation of tea in this country, and which I believe I before noticed, namely, the want of population and labourers. They will have to be imported and settled on the soil, which will be a heavy tax on the first outlay: but this too will rectify itself in a few years; for, after the importation of some thousands, others will come of themselves, and the redundant population of Bengal will pour into Assam, as soon as the people know that they will get a certain rate of pay, as well as lands, for the support of their families. If this should be the case, the Assamese language will in a few years be extinct.

I might here observe, that the British Government would confer a lasting blessing on the Assamese and the new settlers, if immediate and active measures were taken to put down the cultivation of opium in Assam, and afterwards to stop its importation by levying high duties on opium land. If something of this kind is not done, and done quickly too, the thousands that are about to emigrate from the plains into Assam will soon be infected with the opium mania—that dreadful *plague* which has depopulated this beautiful country, turned it into a land of wild beasts, with which it is overrun, and has degenerated the Assamese, from a fine race of people, to the most abject, servile, crafty, and demoralized race in India. This

vile drug has kept, and does now keep, down the population; the women have fewer children compared with those of other countries, and the children seldom live to become old men, but in general die at manhood, very few old men being seen in this unfortunate country, in comparison with others. Few but those who have resided long in this unhappy land know the dreadful and immoral effects which the use of opium produces on the native. He will steal, sell his property, his children, the mother of his children, and, finally, even commit murder for it. Would it not be the highest of blessings, if our humane and enlightened Government would stop these evils by a single dash of the pen, and save Assam, and all those who are about to emigrate into it as tea-cultivators, from the dreadful results attendant on the habitual use of opium? We should, in the end, be richly rewarded, by having a fine, healthy race of men growing up for our plantations, to fell our forests, to clear the land from jungle and wild beasts, and to plant and cultivate the luxury of the world. This can never be effected by the enfeebled opium-eaters of Assam, who are more effeminate than women. I have dwelt thus long on the subject, thinking it one of great importance, as it will affect our future prospects in regard to tea; also, from a wish to benefit this people, and save those who are coming here from catching the plague, by our using timely measures of prevention.

Monthly Outlay of the present standing Establishment.

	Co.'s rupees.		
Superintendent,	500	0	0
1st assistant to ditto,	100	0	0
2d do. do.	70	0	0
1 Chinese black tea maker,	55	11	6
1 ditto assistant to ditto,	11	1	6
1 ditto tea-box maker,	45	0	0
1 ditto interpreter,	45	0	0
1 ditto tea-box maker,	15	8	6
2 ditto green tea makers, at 15:8:6 each,	31	1	0
1 ditto tea-box maker,	33	4	6
1 ditto lead-canister maker,	22	3	0
24 native black tea makers, at 5 each,	120	0	0
12 native green tea makers, at 5 each,	60	0	0
Carry forward,			

Brought forward,			
1 native carpenter,	4	0	0
1 coolie sirdar,	10	0	0
4 mahouts, at 6 each,	24	0	0
4 ditto mates, at 4 each,	16	0	0
Rice for 4 elephants per month,	18	0	0
4 sawyers, at 4 each,	16	0	0
2 Dâk runners, at 3 : 8 : 0 each,	7	0	0
4 Duffadars, at 3 each,	12	0	0
Fixed monthly expenditure in Assam,	1215	14	0
Cash paid to Chinese families in China,	131	2	6
Total monthly expenditure,	1347	0	6

or 16,000 a-year, not including coolies and other items. It should be remembered, that this establishment has been confined to a few tracts as an experiment, and has never been fully worked. The Chinese green tea-makers, canister-makers, and interpreter, have lately been added to the establishment; their services have not, as yet, been brought into account. We are just now availing ourselves of them by making green tea; and, as the natives at present placed under them become available, large quantities of excellent green tea will be manufactured. I suppose two Chinamen might qualify twenty-four natives for the first process; the second, as I have already recommended, might be performed in England, which, in my humble opinion, would effect a great saving, by getting machinery to do the greater part of the work. At all events, it never could be manufactured in Assam without a great expense, and this for want of labourers. However, it is gratifying to see how fast the Chinese acquire the Assamese language; for, after they have been a year in the country, they begin to speak sufficiently well for all ordinary purposes, so that an interpreter can very well be dispensed with. Our Chinamen can speak the Assamese language much better than the interpreter can the English language. They are a violent, headstrong, and passionate people, more especially as they are aware we are so much in their power. If the many behave as do the few, a Thannah would be necessary to keep them cool.

With respect to what are called the *Singpho* tea-tracts, I am

sorry to say we have not been able this year to get a leaf from them, on account of the disturbances that have lately occurred there; nor do I believe we shall get any next year, unless we establish a post at *Ningrew*, which, I think, is the only effectual way to keep the country quiet, and secure our tea. The tea from these tracts is said by the Chinamen to be very fine. Some of the tracts are very extensive, and many may run for miles into the jungles for what we know; the whole of the country is capable of being turned into a vast tea-garden, the soil being excellent, and well adapted for the growth of tea. On both sides of the Buri-Dehing river, as will be seen by the map, the tea grows indigenous; it may be traced from tract to tract to *Hookum*, thus forming a chain of tea-tracts from the Irrawaddy to the borders of China, east of Assam. Ever since my residence at Sudiya, this has been confirmed year after year by many of my Kamptee, Singpho, and Dewaneah acquaintances, who have traversed this route. It is, therefore, important for us to look well to our eastern frontier, on account of our capability to extend our tea-cultivation in that direction. England alone consumes 31,829,620 lb., nearly four laks of maunds, annually. To supply so vast a quantity of tea, it will be necessary to cultivate all the hills and valleys of Assam; and, on this very account, a post at *Ningrew* becomes doubly necessary. A few years hence, it may be found expedient to advance this frontier post to the top of the *Patkai* hill, the boundary line of our eastern frontier. Any rupture with Burmah would add to our tea trade, by taking from them *Hookum* and *Munkoom*, and having the Irrawaddy as our boundary line. These countries are nominally under the Burmese, as they pay a small annual tribute; but this can never be collected without sending an armed force. They are said to be thinly inhabited, the population being kept down by the constant broils and wars which one petty place makes upon another, for the sake of plunder. All the inhabitants drink tea, but it is not manufactured in our way: few, it is said, cultivate the plant. I have, for years, been trying to get some seeds or plants from them, but have never succeeded, on account of the disturbed state in which they live. The leaves

of their tea-plants have always been represented to me as being much smaller than ours.

Muttuck is a country that abounds in tea, and it might be made one extensive, beautiful tea-garden. We have many cultivated experimental tracts in it; we know of numerous, extensive, uncultivated tracts, and it appears to me that we are only in the infancy of our discoveries as yet. Our tea, however, is insecure here. It was but a month or two ago, that so great an alarm was created, that my people had to retire from our tea-gardens and manufacture at Deenjoy and Chubwa, which will account for the deficiency of this year's crop. Things must continue in this state until the government of the country is finally settled; for we are at present obliged, in order to follow a peaceful occupation, to have the means of defending ourselves from a sudden attack, ever since the unfortunate affair at Sudiya. Before the transfer of the tea-tracts in this country can be made, it will be necessary, in justice to all parties, to know if Muttuck is, or is to become, ours or not. The natives at present are permitted to cultivate as much land as they please, on paying a poll-tax of two rupees per year; so that, if the country is not ours, every man employed on the tea will be subject to be called on for two rupees per annum, to be paid to the old Bura Senaputy's son, as governor of the country. This point is of vital importance to our tea prospects up here. Many individuals might be induced to take tea grounds, were they sure that the soil was ours, and that they would be protected and permitted to cultivate it in security.

In looking forward to the unbounded benefit the discovery of this plant will produce to England, to India,—to millions, I cannot but thank God for so great a blessing to our country. When I first discovered it, some 14 years ago, I little thought that I should have been spared long enough to see it become likely eventually to rival that of China, and that I should have to take a prominent part in bringing it to so successful an issue. Should what I have written on this new and interesting subject be of any benefit to the country and the community at large, and help a little to impel the tea forward

to enrich our own dominions, and pull down the haughty pride of China, I shall feel myself richly repaid for all the perils, and dangers, and fatigues, that I have undergone in the cause of British-India tea.

Jaipore, 10th June 1839.

On the Calcareous and Siliceous Microscopic Animals which form the chief component parts of Cretaceous Rocks. By Professor C. G. Ehrenberg.*

In the year 1836, the author communicated to the Academy, that, in the course of his examination of chalk and other limestones, he had found a characteristic feature in the smallest grains of chalk, which, if not identical with, was very similar to crystallisation, and consisted in these appearing as regular elliptical granular-foliated particles. Further researches regarding organic influence on the formation of limestone have afforded him new and remarkable results. The chalk of Puskaresz, in East Prussia; of the Island of Rügen; of Schonen; of the Danish Islands; of Gravesend, Brighton, and Norwich, in England; of Ireland; of Meudon near Paris; and from Cattolica and Castrogiovanni, to the coast at Girgenti, in Sicily, exhibits two different conditions; one inorganic, which is distinguished by its extremely regular elliptical granular-foliated particles, and their fragments or beginnings; and the other organic, consisting of microscopic polythalamiæ. Every thing that has been said of microscopic calcareous animals,—creatures so minute as to appear only like very small grains,—of nautili, or polythalamiæ of the sea-sand and of the tertiary formations,—all that has been said of these, from the period of Janus Plancus, and Soldani, now a century ago, up to the most recent times, is far surpassed by the countless myriads of animals, much smaller in dimensions, and much less visible to the naked eye, which the author has observed forming whole mountains of chalk. Although D'Or-

* From the Reports of the Royal Prussian Academy. Translated from Poggendorff's Annalen, 1839, No. 7.

bigny, Nilsson, Pusch, and other distinguished geological and zoological observers, since the year 1826, have noticed individual larger forms of polythalamic nautili in the chalk of France, Sweden, and the Bukowina; although the author himself (in 1836) proved to the Academy the existence of many calcareous polythalamiæ in the flints of the chalk, which enabled us to infer the presence of prodigious multitudes of such bodies in chalk; and although, in the tabular view printed in 1837, he named these *Rotalites ornatus* (*Lenticulina*, Londsdale), and *Textularia globulosa* (*Discorbis*, Londs.), as the chief forms of the *Polythalamiæ*, and *Cypris Faba?* (*Cytharina*, Londs.), as an *Entomostracon* of the chalk; although, moreover, Mr Londsdale, of London, lately (in 1837) reckoned, with the naked eye, 1000 white granules in a pound of English chalk; yet the numbers and masses of forms, chiefly invisible to the naked eye, which the author of the present paper has very recently observed, though anticipated by him, are very much greater. In these investigations he employed a new and peculiar mode of observation.

As limestone and chalk, by being mixed with water, and magnified 300 times, are seen to contain, besides the granular-foliated particles, likewise coarser opaque particles, which at first seemed to be mere dark parcels of these elliptical granules, or small fragments of larger organisms, the author tried several oils and balsams that increase transparency. These had previously been employed by the author with advantage and success, in the case of infusory animals; but there they were less useful, as they increased the transparency to such an extent as to destroy all light and shade, and also the outlines. But such experiments were eminently successful with the cretaceous animalcules, more especially when turpentine was employed; and of this, the best kind is that obtained from the *Pinus balsamea*, and which is known in the shops by the name of Canada balsam. The use of this substance on finely divided dry chalk, particularly after heat had been applied, distinctly afforded to the author a result which had previously been obscure, viz. that the said chalk contains so vast a multitude of microscopic, and hitherto unknown polythalamiæ, or nautilites, as they have been termed, having a size of from 1-24th to

1-288th of a line, that frequently there must be far above a million in each cubic inch, and hence far above 10 millions in a pound of chalk. In the white or yellow chalk of the north of Europe, the particles of a somewhat crystalline nature, are equal to, if not greater in amount than, the organic remains, according to the respective volumes of the mass; but in the chalk of southern Europe, these organisms, and their visible fragments, greatly predominate; and these consist, as it would seem, exclusively of well preserved polythalamiæ. But when we spoke of a million of polythalamiæ in every cubic inch, we include only those which are well preserved, of which the fourth part of a cubic line, or every 1-12th of a grain of chalk, can be ascertained to contain 150 to 200, which would be equal to 600 or 800 in each cubic line, about 1800 to 2400 in each grain, and from 1,036,000 to 1,382,400 in every cubic inch.

Besides the polythalamic cretaceous animalcules, siliceous infusoria have been found in chalk at Gravesend, near London.* Professor Ehrenberg has also found the cretaceous animalcules in the polishing-slate from Oran in Africa (tertiary marl, according to Rozet), and in the polishing-slate of Zante; and it has been discovered by him, that even the chalk marl of Sicily, which there forms whole ranges of hills, and which Friedrich Hoffmann recognised as a member of the chalk series, is composed of extremely well preserved siliceous infusoria, including several of the characteristic chalk animalcules. He likewise recognised three specimens of similar chalk-marl, composed of infusory animals, or calcareous polishing-slate, among Greek minerals brought by Mr Fiedler; and it has been ascertained that many of the siliceous infusory animals of the chalk-marl of Sicily, Oran, Zante, and Greece, are identically the same, and at the same time do not occur in other localities. Lastly, by an examination of the nummulite limestone of Cahira, and of the pyramids of Gyzeh, specimens of which he brought with him from Egypt, the author has dis-

* In the flints of that locality the author also saw distinct scales of fishes, first of all in the collection of Mr Bowerbank of London; but he afterwards himself found similar specimens, which he carried home with him.

covered that they contain the microscopic animalcules of the chalk of northern and southern Europe.

It appears that the following conclusions may be deduced in reference to the whole subject.

1. Many, probably all the chalk rocks of Europe, are *the product* of microscopic, spiral, coralline animalcules, which, for the most part, are quite invisible to the naked eye, some provided with calcareous, and others with siliceous shells, and which are from 1-24th to 1-288th of a line in size.

2. The chalk rocks of Southern and Northern Europe contain, among their component parts, many perfectly similar calcareous animals, of which the most remarkable are, *Textularia globulosa*, *Textularia aciculata*, and *Rotalia globulosa*. *Rotalia ornata*, *Glabigerina bulloides* (D'Orbigny), *Planulina turgida* and *sicula*, *Rosalina globularis*, *Textularia aspera*, *brevis*, *dilatata*, and *italica*, together with *Escharella scutellaris* (*Eschara scutellaris*, Soldani), are the remaining *characteristic* forms of the chalk, to which are also to be added some of the species mentioned by D'Orbigny, Nilsson, and Pusch, and the *Spirulina* of Lord Northampton.

3. The cretaceous districts bordering the Mediterranean in Sicily, Barbary, and Greece, and generally regarded as tertiary deposits, are therefore, judging from their organisms, to be considered as chalk or secondary rocks; and the nummulite limestone of Egypt is to be viewed in the same light.

4. The chalk strata of southern Europe, round the basin of the Mediterranean, are distinguished from those of the north and east of Europe by their better preserved chalk animalcules, and by the smaller number of their elliptical grains; and the converse holds good.

5. The chalk strata of the south of Europe contain few or no flints. Those of the north present many extremely regular horizontal beds, which are often only from one to six feet separated from one another. This feature was previously well known; but, what seems new and remarkable, is the observation made by the author, that in the chalk strata of northern Europe, hitherto no marls of infusory animals have been found like those which alternate with the chalk in such prodigious abundance in Sicily, Oran, and Greece. A comparison of the

southern infusory-marls, and of the northern beds of flint, at once presents itself to the mind. Thus, by means of this changed relation, the formation of flint would receive its full explanation. A greater age, as deduced from the conversion of the infusory marl-beds into flint-layers, and from the greater decomposition of the calcareous animals, and their conversion into inorganic particles, might thus be assigned to the chalk strata of the north; but still, local circumstances might produce different effects at the same periods, as is, from other circumstances (from the similar chalk animalcules, &c.), more probable.

6. The want of numerous and varied forms of siliceous infusory animals in the chalk previously noticed by the author, has now disappeared, and, in its place, great abundance has presented itself.

In all, the author has observed seventy-one different microscopic calcareous and siliceous species of animals in the chalk; but, besides these, also numerous larger calcareous animals (1-24th of a line in size) and many included plants, Tethyæ, Sponges, Confervæ, and Fuci. The varied forms of the genera *Rotalia* and *Textularia* of the Polythalamiæ, appear to him to constitute the great mass of the chalk of all localities. He reckons altogether seven genera and twenty-two species of polythalamic microscopic calcareous animals; and, moreover, microscopic and larger nummulites, cypridæ, &c. Further, he has hitherto determined forty species of siliceous infusory animals which belong to fourteen genera, without including the eight forms previously enumerated, and which were probably soft, and merely included in flint. He has found five species of plants containing silica. In the flints of the Jura limestone of Cracow, he detected well preserved peculiar Polythalamiæ, and remains of Sponges or Tethyæ; and lately, he has found Polythalamiæ of the chalk in the flints occurring in the gault which lies under the chalk at Cambridge in England.

A general table of these relations of the animals from the chalk and chalk-marl of the fourteen localities observed by him, and also specimens of the rocks, together with a collection of well-preserved microscopic preparations, containing

nearly a perfect series of the different species of animalcules, were exhibited to the Academy.

To this paper Professor Ehrenberg added a preliminary summary of his examination of the Spiral-corals or Polythalamiæ, considered in a zoological point of view.

On the Extinction of Human Races. By Dr Pritchard.

While other branches of natural history are diligently cultivated, no degree of general attention proportionable to its importance is given to Ethnography, or the natural history of the human races. It has appeared to me, for the following reason, desirable to call attention to this remark. Opportunities for pursuing the investigation to which I have alluded are every day failing and disappearing for ever. This is not the case with respect to other subjects in natural history or general science. We might contrast the different departments of human knowledge in reference to the durability of the objects with which they are conversant. Mathematical science properly so termed, contemplates only eternal and immutable relations. The truths which it brings to light must have been recognised, precisely as they are now known, from all eternity, or where and whenever there were intelligences capable of apprehending them. The abstract relation of number and space must indeed have been the same, had no intellect whatever existed in the universe; but, as far as mankind are concerned, it may perhaps be said, that the facts which relate to inorganic nature are eternal truths, for matter, as such, is to our view indestructible, as it is ingenerable and immutable in its properties. Perpetuity of existence, so far as our ideas and knowledge of time extend, belongs alike to the smallest molecules of the unorganized world, and to those great masses which, revolving in the heavens age after age, have offered themselves to the view of countless generations. It is quite otherwise with all the productions of organic nature, which exist only in series of similar individuals, destined, one after another, to originate and decay, a destiny common alike to the lords of the creation, and to the potherbs and legumes which contribute to their daily food. But the organized world is

subject to another vicissitude. The tribes of which it consists have but a definite existence, determined by the conditions of external nature. When these have changed, it would appear that new organized tribes, adapted by their physical constitution to the new state of external circumstances, replace the old ones. It would be interesting to inquire, whether such changes have had any influence on the destiny of the ancient races of men? Certain it is, that many vast regions of the earth, if not the whole or the greater part of its surface, were formerly the abode of tribes which have long ago perished; and many of these races were different in physical character from those which at present exist in the same countries. Were the old races swept away, by changes in climate and local circumstances which became incompatible with the conditions of their existence? This is a question, for the solution of which no satisfactory data can be found.

An opinion was entertained some time since in France, which, if it had been well-founded, would perhaps have furnished an example of this kind. A prize was offered by the Geographical Society of Paris for an essay, which was to include the history of ancient Negro races, supposed to have been the aboriginal inhabitants of the region of Central Asia, of the great Himalaya and the mountains of Kuen-lun to the northward of that chain. It was fancied, that the existence of such a people in that quarter was satisfactorily attested by ancient Chinese historians; and if the fact had been established, that the highest and coldest region in the Old World was the aboriginal abode of black, woolly-haired tribes, it might safely have been inferred, that some great physical change must have taken place, since Negroes now exist scarcely, in their wild and native state, except in the hottest part of the intertropical clime. But it turned out, that this notion was entirely founded on mistake, and that the Kuen-lun, recorded in ancient Chinese books to have been the abode of Negroes, was not the mountainous country bearing that name, which is to the eastward of China and northward of Tibet, but certain islands, bearing the same designation, in the Indo-Chinese seas. It is uncertain to what causes the ancient depopulation of great regions of the earth may be attributed; such catastrophes appear, however, to have

been of great extent. In the remotest parts of Northern and Eastern Asia, remains have been discovered, which prove that nearly the whole of that great continent was once occupied by human races whose very names have perished. Some of these nations appear to have made some progress in arts and civilization. Their tombs are found spread over the countries eastward of Jenisey. In those remote countries, such remains are in great numbers and of magnificent construction; they contain ornaments and various implements of silver, gold, and copper; there are girdles of the precious metals, bracelets decked with pearls. In the same tombs are fragments of earthen manufactory or porcelain. I shall not at present detail the arguments which have satisfied the learned academicians of St Petersburg, that such relics belonged to races of people long since extinct, who must have disappeared before the light of history dawned upon these countries, but shall hasten to remark, that discoveries leading to a similar result have been traced through the New World, from the countries bordering on the Mississippi and Ohio, where tombs are found containing skeletons of a different conformation from that of the present tribes, to the high table-land of Titicaca in the Peruvian Cordillera, where these strangely formed skulls are seen which deviate so widely from the usual form of human heads. Even in the islands of Polynesia vestiges are discovered, which have been referred to a former race of inhabitants; and, to conclude this series of observations, there are facts which indicate that extensive countries in Europe were occupied by races of a different physical character from the present natives, in times which preceded the arrival of Celts and Goths, and other Indo-Germanic nations from the East.

I shall now pass to another view of the subject, for the sake of introducing which, I have been led to offer these remarks. The extermination of human races is still going on. Whatever were the causes which destroyed the ancient tribes, we know to what agency we are to attribute the similar fate of many nations who have perished since the historic age commenced. How many whole races have become extinct during the few centuries which have elapsed since the modern system of colonization commenced? The Guanches, the numerous peo-

ple of the Canary Islands, now exist only in their mummies, and in the traces of those arts by which they sought to procure for the dead an eternal repose, and which have proved the occasion of transporting these mummies into the museums of Europe. It would be endless to recount the names of tribes and whole nations in America, who have been extirpated by the Spanish conquerors of that country. The last race that was utterly destroyed was the Charreas, of whom I saw three surviving individuals brought to Paris, and exhibited as curiosities. They were a most singular race of people, whose moral and physical character is briefly sketched by Don Felix de Azara, but of whom we have no satisfactory account, nor can we now ever hope to obtain it. The whole country now occupied by civilized nations in the New World, was, three or four centuries ago, thickly peopled by native tribes. A similar process of extermination has been pursued for ages in South Africa, formerly the abode of numerous pastoral nations of Hottentots, a peaceable and inoffensive race, who wandered about with numerous flocks, in a state of primitive simplicity, and whose descendants are now found in the miserable and destitute Bushmen, condemned to feed upon vermin and reptiles, and rendered savage and cruel by the wretchedness which their Christian conquerors have entailed upon them. Wherever Europeans have settled, their arrival has been the harbinger of extermination to the native tribes. Whenever the simple pastoral tribes come into relations with the more civilized agricultural nations, the allotted time of their destruction is at hand; and this seems to have been the case from the time when the first shepherd fell by the hand of the first tiller of the soil.

Now, as the progress of colonization is so much extended of late years, and the obstacles of distance and physical difficulties are so much overcome, it may be calculated that these calamities, impending over the greater part of mankind, if we reckon by families and races, are to be accelerated in their progress; and it may happen that, in the course of another century, the aboriginal nations of most parts of the world will have ceased entirely to exist. In the mean time, if Christian nations think it not their duty to interpose and save the nu-

merous tribes of their own species from utter extermination, it is of the greatest importance, in a philosophical point of view, to obtain much more extensive information than we now possess of their physical and moral characters. A great number of curious problems in physiology, illustrative of the history of the species, and the laws of their propagation, remain as yet imperfectly solved. The psychology of these races has been but little studied in an enlightened manner; and yet this is wanting, in order to complete the history of human nature, and the philosophy of the human mind. How can this be obtained, when so many tribes shall have become extinct, and their thoughts shall have perished with them? I cannot conclude this paper without making an appeal to the members of the British Association in behalf of an attempt, which has been lately set on foot by individuals, prompted by the most noble and generous sentiments, to do something more than merely to record the history of the perishing tribes of the human family, and to take up seriously the consideration, whether any thing can be done effectually to prevent the extermination of the aboriginal tribes.*

On the Geographical Distribution of Insects. (Continued from vol. xxvii. page 351.)

It is known that insects both of a creophagous and saprophagous nature, gradually diminish in relative numbers from the poles to the equator; but it remains to be seen whether they increase in the same ratio when viewed separately. This will appear from the following table:—

	Creo-thalerophagous Species are to the whole Creophaga,	Creo-saprophagous Species
New Continent.		
North America, . .	: : 1 : 5,26	: : 1 : 16,52
South America, . .	: : 1 : 12,29	: : 1 : 45,83
Old Continent.		
Lapland and Siberia, .	: : 1 : 4,17	: : 1 : 9,58
Europe, . . .	: : 1 : 5,30	: : 1 : 11,40
Africa, . . .	: : 1 : 6,22	: : 1 : 25,08
Oceania, . . .	: : 1 : 10,56	: : 1 : 46,20

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* From Longman's Chronicle for December 1839.

It thus appears that the creo-saprophagous species diminish from the Poles to the Equator in an infinitely more rapid manner than the creo-thalerophagous, and nearly in the same ratio in both Continents. The rarity of these insects has been long since remarked in inter-tropical America, and a similar observation was made at a still earlier period, by Dr Horsfield, in relation to Java. This author alleges as the cause, that the ants and termites have taken full possession of the ground; but why should these insects have destroyed the Necrophaga rather than the Carabidæ, which likewise live on the surface of the ground, and of which a considerable number are found in Java? The scarcity in question may be much more plausibly ascribed to the too rapid decomposition of carcasses and other animal substances, as has been stated above.

It is more difficult to form a similar estimate in regard to the phytophagous Coleoptera, because their thalerophagous and saprophagous species are mingled in the same families, and there are many with the nature of whose food we are imperfectly acquainted. Besides, the Saprophaga comprehend no small portion of Xylophagi and Brachelytra, which are so much neglected by collectors on account of their small size, that we are scarcely acquainted with any inhabiting countries out of Europe. No calculation of this kind, for the eight regions established above, would therefore lead to any satisfactory issue. But the attempt may be made for the different countries of Europe, of which the entomology is best known, and the following is found to be the result:—

	Number of Phytophagous Species.	Phyto-saprophagous Species are to the entire amount of Phytophaga,—
Lapland, . . .	529	: : 1 : 261
Sweden, . . .	1438	: : 1 : 219
England, . . .	2622	: : 1 : 180
France, . . .	3276	: : 1 : 188

From this it may be presumed that, contrary to what takes place with the creo-saprophagous Coleoptera, the phyto-saprophagous species go on increasing from the Poles to the Equator; it is remarked, indeed, that the Coprophaga and Longicornes

are singularly augmented in number as we approach more southern countries. This circumstance, moreover, is in harmony with the increase of vegetables in the same direction, and the enormous mass of detritus which they deposit on the ground in intertropical regions, detritus which is not of such a nature as to disappear rapidly, like dead animal matter, under the action of the solar rays.

On reviewing all the species of all the orders existing in England, Kirby and Spence estimate the Phyto-saprophaga of that country to be to the Phyto-thalerophaga as 1 : 9. Pushing their calculations further, these skilful entomologists conceive that, among the latter, the fungivorous species are about $\frac{1}{20}$, and the granivorous $\frac{1}{25}$; and that, among the Phyto-saprophaga, the half are lignivorous, and $\frac{1}{3}$ coprophagous. These proportions nearly correspond to those obtained by M. Lacordaire for the insects of France.

On the extent of the habitation of Species, Genera, and Families.—After having studied the relative proportions of the various entomological groups, it is natural to inquire as to the space which each of them occupies on the globe, for all have their limits, beyond which they never extend. The space comprised within these limits constitutes what is called their *area*. This area is *continuous* when all the countries composing it are contiguous to each other, and form an uninterrupted whole; *disjoined*, when the contrary is the case. These terms, however, must not be taken too literally; and when two countries are separated only by a small interval, as the most part of islands are from the continent to which they belong, they ought to be considered as united. Their vicinity permits species easily to pass from one to the other, and it is very probable that they would be found in the intermediate places, if any such existed. Thus, Madagascar could not, with any propriety, be separated from the coast of Africa, opposite to which it lies; and it is found that a great number of species are common to both.

Mountains are very often the cause of the disjunction of areas, and may be regarded as islands in the middle of continents.

Continuous areas are most common, but they do not afford so many interesting considerations as the other sort. Disjoined areas become more rare, in proportion as we descend to inferior groups, and acquire great importance when we arrive at species, for if the individuals of any one of them exist in two countries remote from each other, we are often greatly perplexed to explain the fact; in such a case we are almost obliged to admit that this separation has taken place with the origin of that species, in other words, that it must have had a multiplied origin.

A group is said to be *sporadical* when it inhabits many regions, and *endemical* when found in one only. It will be observed that these words have only a relative meaning depending on the extent assigned to regions.

Notwithstanding their powers of locomotion, entomological species appear less sporadical than vegetable. About $\frac{1}{7}$th of the phanerogamous plants of the United States reappear in Europe, and the proportion is much higher in regard to the cryptogamia, while it falls greatly short of this in the case of insects. The reason of this no doubt is, that the seeds of plants are liable to be transported to a distance by a multitude of causes, which is not the case with the eggs of the latter. But locomotion plays an important part in their sporadicity, when they are compared with each other. We find that it is the Lepidoptera which furnish the greatest number of sporadical species, and after them the Hymenoptera.

The kind of nourishment also exercises no small influence on this phenomenon. Phytophagous species are more remarkable in this respect than creophagous, although there are a considerable number of the latter whose geographical extension is very great.

It happens, as might naturally be expected, that species extend more in a parallel direction than in the opposite one. The temperature is much more uniform in the former than in the latter case. Thus we find at Japan a great number of the kinds which occur in the neighbourhood of Paris; Thunberg has mentioned 50 in his catalogue of Japanese insects of which this holds true. The same thing is observed in all latitudes,

for there are species (*Pap. Demoleus* and *Epius*; *Nymphalis Bolina*, &c.) which extend from the western coasts of Africa under the equator, as far as the Islands of the Indian Archipelago, and even the eastern side of New Holland.

This rule, however, is not without numerous exceptions, and we are acquainted with many species whose habitation in respect to latitude is extremely extended. *Dytiscus marginalis*, very common in Greenland, reappears on the coasts of Barbary, and many Phalenæ and Noctuidæ of the vicinity of Paris, and even of the north of Europe, exist at the Cape of Good Hope. It is observed, however, that the habitation seldom extends directly in the line of the meridians; it has a tendency, in regard to European species, to incline to the east, insomuch that they gain as much in one sense as in the other. Thus we find *Dytiscus griseus* extend from Lapland to Bengal; *Polyommatus bœticus* of the environs of Paris to the isles of Sunda; *Limenitis aceris* of Hungary, to Java; the Sphinges *Celerio* and *Nerii* of the middle of France to the Mauritius; the *Sphynx convolvuli* of the north of France as far as the island of Polynesia, to Taity among others, where it is not rare. The new continent also presents many similar examples. A great number of its Lepidoptera (*Vanessa huntera*, *larinia* and *jatrophæ*, *Pap. thoas*, *Nymphalis acheronta*, *Argynnis columbina* and *vanillæ*, &c.) are distributed from Southern Brazil as far as the vicinity of New York.

In all the preceding instances the areas are continuous; if we wish for striking examples of interrupted areas, they must be sought among the species common to both the New and Old Continent, their insulation being the most complete to be found on the globe. The list of these species is pretty copious, and it is somewhat remarkable that all are European. We are hitherto unacquainted with any insect essentially African or Asiatic which reappears in America, and if that vast continent do possess any in common with these two countries, they belong to those eminently sporadical species, which exist almost everywhere, such as *Vanessa cardui* already alluded to.

According to Latreille, the majority of the species of Greenland are the same as those of Lapland, so that this country,

American by its geographical situation, would in reality be European by its entomology. Canada and the north of the United States possess, in like manner, a multitude of our species. We there find *Brachinus crepitans*, *Dermestes murinus* and *vulpinus*, *Vanessa antropa*, *V. album*, *cardui* and *atalanta*, *Polyommatus argiolus*, *Hesperia comma*, *Zygæna onobrychis*, *Vespa vulgaris*, *Ophion luteus*, *Pentatoma juniperina*, *Cercopis spumaria*, *Helophilus pendulus*, &c, &c. Mexico has a much smaller number, for hitherto there have only been found *Vanessa atalanta* and *Cardui*, at Vera Cruz, and *Dermestes vulpinus*, in California. Finally, we are unacquainted with any other of our insects in the whole of South America than *Vanessa cardui*, *Corynetes* and *Pristonychus*, of which we have already spoken.

In general, it is not the richest genera which furnish the most sporadical species, as the above examples prove, since they are taken for the most part from genera not very numerous in species. We have only further to examine the great genera *Cicindela*, *Carabus*, *Hister*, and *Chrysomela*, to be convinced of the truth of this assertion; but it cannot be made without the admission that there are pretty frequent exceptions, particularly among the Lepidoptera.

It might be supposed also, that the sporadicity of species is in direct relation with that of genera; that is to say, that the most sporadical species are found in the most sporadical genera; but the exceptions in regard to this are so numerous, that it is not easy to say on which side the rule lies. The sporadicity of the two groups in question rests, in truth, on different foundations. That of genera appears to be in relation with the number of their species, while that of species has no connection with the number of their individuals; and it is very easy to conceive an eminently sporadical genus, which may be composed of species peculiarly endemical, but numerous and every where diffused. This case, however, does not seem to be exemplified in nature.

It is scarcely necessary to direct attention to the fact, that the inverse takes place in the reciprocal endemicity of genera and species, and that we cannot conceive an endemical genus

composed of sporadical species. Such a genus cannot be of the latter character, because its species have a limited geographical extension; but there is nothing to prevent it being rich in species, which in such an event are accumulated in a more or less restricted district. This is what takes place with many intertropical genera especially, which thus constitute so many exceptions to the preceding rule, that genera are so much the more sporadical as their species are more numerous.

Being unable, in the present state of entomology, to calculate the area of the different groups, as rigorously as is done by botanists in regard to the objects of their study, M. Lacordaire has attempted to give a view of the distribution of the genera of each family of the Coleoptera, considering them under different aspects, which enables us to perceive their greater or less degree of sporadicity, and the progression they follow from the poles to the equator. For this purpose De Jeans' Catalogue has been again employed, which contains 2211 genera, sufficient materials to operate upon with some degree of exactness. It is of little importance to the inquiry, whether these genera be all subsequently adopted or not; it is enough if they indicate differences of form, however minute, and that these differences be proper to such and such countries. The rapidity with which entomological discoveries succeed each other, gives us reason to suppose, that, in future, their number will be increased rather than diminished.

By a preliminary proceeding analogous to one of those employed by botanists, an effort has been made to determine the number of regions which the genera of each family inhabit. Only, instead of taking as a basis the rather numerous regions established on a former occasion (which would have been impossible, since we do not know the habitation of the different groups with sufficient exactness), the number has been limited to eight, which has already served for some previous calculation. In this way the following table has been obtained.

Families.	Total of Genera.	No. of Genera proper to								Proportion to 100 Genera.	
		Regions 8.	Regions 7.	Regions 6.	Regions 5.	Regions 4.	Regions 3.	Regions 2.	Region 1.	Endemical.	Sporadical.
Carabidæ, . .	183	6	2	8	7	11	20	24	105	57	43
Hydrocanthares,	26	...	1	4	5	1	7	3	5	19	81
Brachelytres, .	72	...	1	3	3	2	17	14	32	44	56
Sternoxes, . .	114	1	2	2	3	15	15	22	54	47	53
Malacodermes,	54	1	...	1	5	2	9	9	27	50	50
Teredyles, . .	31	1	...	...	2	4	10	3	11	35	65
Clavicornes, .	63	1	...	4	5	7	7	11	28	44	56
Palpicornes, .	11	1	...	2	1	...	2	4	1	9	91
Lamellicornes,	197	3	3	5	7	9	11	33	126	63	37
Malasomes, .	124	...	1	1	...	2	14	22	84	67	33
Taxicornes, .	54	...	...	...	2	6	6	8	32	59	41
Tenebrionites, .	56	...	1	2	...	...	3	12	38	67	33
Helopidæ, . .	35	...	...	2	2	2	1	4	24	68	32
Trachelides, .	26	...	..	2	2	1	3	6	12	46	54
Vesicants, . .	19	...	...	1	3	2	2	3	3	42	58
Stenelytres, .	14	...	...	1	...	2	1	3	7	50	50
Curculionidæ, .	350	1	3	1	12	17	17	59	240	68	32
Xylophages, .	56	...	1	1	4	7	9	8	26	46	54
Longicornes, .	348	1	1	1	3	12	17	47	266	76	24
Chrysomelidæ,	328	1	3	3	10	11	29	49	222	67	33
Trimeres, . .	38	...	1	2	1	4	2	6	22	57	43
Dimeres, . .	12	...	..	...	...	...	2	...	10	83	17
	2211	17	20	46	77	117	204	350	1380	62	38

It appears from this table that the sporadicity of genera is less than might be supposed; the result would no doubt have been different, if the number of regions were more considerable, but the fact would remain not less true upon the whole. It will be observed also that the Carabidæ, which constitute of themselves at least nine-tenths of the creo-thalerophagous genera, are those whose geographical extension is most considerable; but this holds true only among the coleoptera. When the whole class is taken into account, the phytothalerophaga have an indisputable superiority.

The next interesting inquiry was to ascertain in what proportion all these genera are distributed among the eight regions above noted; this is shewn by the following table, which presents the total number of the genera of each family found in every one of these regions, that of the genera proper to each, and the proportion existing between these two numbers.

Families.	North Amer.—Mexico.			South Amer.+ Mexico.			Africa.			Europe.			Lapland and Siberia.			Asia.			Indian Archipelago and Oceania.			New Holland.		
	Total No. of Genera.	Genera peculiar to	Proportion. :: 1 :	Total No. of Genera.	Genera peculiar to	Proportion. :: 1 :	Total No. of Genera.	Genera peculiar to	Proportion. :: 1 :	Total No. of Genera.	Genera peculiar to	Proportion. :: 1 :	Total No. of Genera.	Genera peculiar to	Proportion. :1 :	Total No. of Genera.	Genera peculiar to	Proportion. :: 1 :	Total No. of Genera.	Genera peculiar to	Proportion. :: 1 :	Total No. of Genera.	Genera peculiar to	Proportion. :: 1 :
Carabidæ, ...	61	7	8.7	76	30	2.5	82	26	3.1	69	18	3.8	29	3	9.6	48	1	48.	27	5	5.4	16	1	16.
Hydrocanthares,	16	...	...	13	...	...	10	...	...	19	2	9.5	11	1	11.	11	...	...	10	1	10.	2	...	...
Brachelytres,....	34	...	...	24	9	2.6	13	1	1.3	54	20	2.7	26	2	13.	5	1	5.	4	...	...	1	...	...
Sternoxes,.....	38	1	38.	55	26	2.1	25	5	5.2	40	5	8.	14	...	...	17	1	17.	15	...	...	9	5	1.8
Malacodermes,..	24	1	24.	33	15	2.2	12	1	1.2	20	4	5.	7	...	...	10	...	...	10	...	...	5	...	...
Teredyles, ...	18	2	9.	15	3	5.	12	3	4.	19	1	1.9	3	...	...	9	...	...	3	...	...	2	...	...
Clavicornes,.....	36	2	18.	29	4	7.2	19	1	1.9	46	12	3.8	13	...	...	8	...	...	5	2	2.5	6	1	6.
Palpicornes,.....	5	...	...	5	...	...	6	...	...	10	1	10.	5	...	...	5	...	..	3	...	...	1	...	...
Lamellicornes,..	42	3	14.	92	49	1.8	83	33	2.5	42	8	5.2	11	1	11.	43	1	43.	28	2	14.	26	14	1.8
Melasomes, ...	7	2	3.5	36	29	1.2	65	28	2.3	36	4	9.	10	1	10.	32	8	4.	9	1	6.	4	3	1.3
Taxicornes,.....	14	...	...	25	12	2.	12	2	6.	25	5	9.	2	...	...	6	...	...	12	5	2.4	3	2	1.5
Tenebrionites,...	11	...	...	20	12	1.6	14	6	2.3	19	6	3.1	5	...	...	7	2	3.5	11	8	1.3	3		3.
Helopidæ,..	9	1	.9	19	14	1.3	10	3	3.3	7	2	3.5	4	...	...	5	...	...	7	...	...	4	2	2.
Trachelides,.....	12	1	12.	9	2	4.5	7	2	3.5	15	4	3.7	10	1	10.	4	1	4.	7	1	7.	1	...	...
Vesicants,.....	6	...	...	10	4	2.5	12	4	3.	11	1	11.	4	...	...	6	...	...	1	...	...	...	...	...
Stenelytres,.....	2	1	2.	4	...	...	1	...	...	13	4	3.2	6	...	...	2	...	...	1	...	...	2	...	...
Curculionidæ,....	67	4	16.7	168	100	1.6	82	35	2.3	102	38	2.6	34	3	11.3	50	12	4.1	50	15	3.3	35	15	2.3
Xylophages,.....	28	1	28.	28	8	3.5	18	2	9.	35	10	3.5	10	...	...	7	1	7.	3	...	...	...	...	...
Longicornes,.....	58	8	7.2	203	160	1.2	73	42	1.7	50	11	4.5	20	2	10.	35	8	4.3	40	15	2.6	18	10	1.8
Chrysomelidæ,..	68	4	17.	190	134	1.4	74	21	3.5	65	10	6.5	33	...	...	48	5	9.6	53	17	3.1	22	6	3.6
Trimeres,.....	13	1	13.	18	8	2.2	12	4	3.	17	6	2.8	6	...	...	5	...	...	7	...	...	3	...	...
Dimeres, ...	2	..	...	2	1	2.	1	...	...	11	9	1.2	...	...	...	...	...	...	...	...	...	...	...	...
	571	39	14.3	1074	619	1.7	643	219	2.9	715	181	3.9	263	14	18.7	373	41	9.1	303	72	4.2	163	60	2.7

If we deduct the three last mentioned regions, Asia, the Indian Archipelago, and New Holland, the entomology of which is not sufficiently known to enter into a very exact calculation, we cannot fail to be struck with the rapidity with which the genera proper to each region increase, the nearer we approach the Equator. This is distinctly seen by comparing Siberia, Europe, and Africa, with each other, and the proportion is equally maintained in the New Continent. Genera, consequently, are more endemical the more southern the region to which they belong; or, which is the same thing, their sporadicity diminishes from the Poles to the Equator; and, as it has been already shewn that the endemicity of species is in direct relation with that of genera, this conclusion extends equally to them. This fact, it may be remarked, is not peculiar to insects, but common to all other organised beings, for they are much more tolerant of an increase than a diminution of heat. We thus see the northern species, in general, advance much farther south than the intertropical kinds do in an opposite direction. The exceptions to this are chiefly found in the classes which nature has protected against the changes of temperature by hair or feathers, and among such as live in the water, which are less sensible of the extremes of heat and cold.

The table shews, besides, the immense superiority of South America over the other regions, in respect to genera peculiar to it; and this holds true even with the Carabidæ, of which it possesses a much smaller absolute amount than Europe. If the reader feel desirous to compare the Old and New Continent in this point of view, the following table, which presents the number of genera proper to each, as well as their common genera, affords the means of doing so. It will be seen that the first, although very inferior to the second in surface, yields very little to it in the light we are now considering. The pretty numerous genera, common to them both, almost all belong to the northern and temperate regions.

Families.	Genera peculiar to		Genera common to both Continents.
	the New Continent.	the Old Continent.	
Carabidæ	52	81	50
Hydrocanthares . .	1	8	17
Brachelytres . . .	11	39	22
Sternoxes	35	45	34
Malacodermes . . .	22	15	17
Teredyles	6	9	16
Clavicornes	7	21	35
Palpicornes	...	6	5
Lamellicornes . . .	58	104	35
Melasomes	36	85	3
Taxicornes	15	26	13
Tenebrionites . . .	12	38	6
Helopidæ	17	12	6
Trachelides	4	11	11
Vesicans	5	9	5
Stenelytres	1	9	4
Curculionidæ . . .	130	173	47
Xylophages	12	18	26
Longicornes	193	126	29
Chrysomelidæ . . .	166	121	41
Trimeres	10	19	9
Dimeres	1	9	2
	794	984	433

These figures, which express the numerical relations of groups to each other, by no means afford an exact idea of their real distribution. A map alone could do this, if it were possible to construct one. In fact, every group (with a few exceptions), whatever may be its importance, has a certain point of the globe where it becomes dominant, that is to say, where its elements are assembled in greater numbers than in any other place; on leaving this centre, it sends out rays or branches in different directions, which are the more numerous, and extend in general to greater distances, the higher in value the group happens to be; in other words, the ramifications of a family are more prolonged than those of a tribe, those of a tribe than those of a genus, and so on. These branches are immediately connected with the trunk when the area is continuous, or they are separated from it, or even occasionally broken into numerous parts, when it is disjoined. Each group, besides, has what may be called its strayed children, which are found widely insulated from their associates, and wandering among other groups with which they have no affinity in

form. Sometimes a group may have all its elements concentrated on a single point of greater or less extent; this is the case with such as are endemical, at other times they may be dispersed over so extensive a territory, that it would be difficult to say where the group has its metropolis; this takes place with the kinds which are eminently sporadical. If we suppose these groups to cross and intermingle with each other in every way over the surface of the globe, which they embrace as a net, it will be understood that although we may in some degree conceive such a picture, it is impossible to render it visible to the eyes.

Every family, however poor it may be in species, furnishes a proof of what has just been stated; that of the Carabidæ, one of the best known, may be taken as an example. All are aware that its province extends over the northern and temperate parts of the Old Continent, where it occupies a zone comprised nearly between the 68° and 43° north latitude. Thence its ramifications extend over all the globe, ceasing only where vegetable life terminates: but its tribes for the most part shew a tendency to establish themselves in other places than where it has its principal seat, some of them at considerable distances from it. The Simplicipedes, Harpalidæ, and Sabulipalpes, are the only tribes which have the greater number of their species in the zone above indicated; the Feronidæ and the Truncatipennes seem to be nearly equally divided among the different regions of the globe; the Cicindelidæ have only two genera (*Cicindela* and *Megacephala*) in the zone in question; all the rest are proper to intertropical regions, and almost equally shared between the Old and the New Continent; finally, the Scaritides and the Patellimanes have their principal seat in the warm countries of the old world. When viewed, however, in groups inferior to that of a family, the greater part of these tribes yield to the latter in respect to their sporadicity. Such in particular is the family of the Scaritides, which, if we were to deduct the genus Clivina, which extends to the Arctic Polar circle, would be deficient throughout the old world beyond the 45° north latitude. On arriving at the genera, the matter becomes still more obvious. Some of these appear eminently sporadical, although

but moderately rich in species, such as the genus Calosoma, which contains scarcely more than thirty, sixteen of which are allotted to America, ranging from the Arctic Regions to Terra del Fuego, and fourteen to Africa, Europe, Siberia, China, and New Holland, where they are scattered about often at immense distances from each other. Among the richest, there is one (*Cicindela*) which covers the whole globe with its species ; but these, although more abundant in warm countries, are pretty equally spread, and our climates possess a considerable proportion ; their area is likewise continuous, except in the case of the two continents. Other genera shew a very different mode of distribution, as for example the genus Carabus. Its species, as is well known, are concentrated in the same zone as the family, and in the Old Continent they cease at the foot of the Atlas and the coasts of Asia Minor. In the New Continent, on the contrary, we find fifteen concentrated in Canada and the North of the United States ; then a single one (*C. Chilensis*, Eschs.) in the south of Chili ; and, finally, another *C. Basilicus*, Chev.*), among the Antilles, at Porto Rico, where it was lately discovered. This genus likewise presents, in an eminent degree, a triple example of sporadicity, of the disjunction of areas, and of species insulated from their groups. Finally, there are numerous other genera of the same family, which are peculiar to countries, and even localities, of a very limited extent.

It is useless to enter into analogous details in relation to species ; it is obvious that their geographical extension in general is inferior to that of genera, although in other respects they repeat, as to continuity and disjunction of areas, what is observed in the preceding groups.

The distance of regions from each other likewise produces general results, which it is necessary to take into account.

When two regions which resemble each other are at the same time contiguous, their species are almost wholly alike ; this is witnessed, for example, in France and Germany. If these regions are separated by a considerable interval, their species are for the most part different, but the genera remain the

* Magazin de Zoologie de Guerin, Insectes, pl. 169.

same; it is thus that Spain and the coasts of Syria, or, if we desire a still more striking example, France and the parts of Siberia situate under the same parallel, possess a multitude of common genera with species almost precisely similar. Finally, if these regions are separated by an immense interval and by natural barriers, nothing continues common to them but the families. Thus Tucuman and the shores of the Caspian Sea have the greatest resemblance to each other in respect to soil, which is arid, saline, and abounding in plants producing soda; both are likewise very rich in melasomes, but not one either of the genera or species is identical.

It is this diversity, produced by distance, which gives to each region its proper entomology; but at the same time the country reacts, in some degree, on the species, by giving them a peculiar *facies*, which reveals their origin often at a first glance. An entomologist, with an experienced eye, will frequently be able to say of an insect presented to him, and which he had not before seen, whether it be American, African, or Asiatic, as readily as a botanist can do the like, in most cases, with plants; but it will be understood that this intuition is not of such a nature as to be explained by words.

(To be concluded in next Number.)

On the Inflammable Gases of the Prussian Coal-Mines. By Professor Gustav Bischof.*

The Financial Department of our Ministry intrusted to me the investigation of the inflammable gases occurring in the *Prussian* coal-pits, and directed me also to make experiments on the action of Davy's safety-lamp in explosive atmospheres. During the last autumn, I collected inflammable pit-gas, perfectly pure, as it issues from the fissures in *Gerhard's* level, and in an old abandoned level at *Wellesweiler*. At the same time, by means of an apparatus, into which I could at pleasure introduce explosive mixtures of various degrees of inten-

* From a letter addressed to Privy-Councillor Von Leonhard. Translated from Leonhard and Bronn's *Jahrbuch* for 1839, part 5.

sity up to the most powerful mixture for detonation, I have tried the action of the safety-lamp, and convinced myself that for the miner it is a real talisman. Extracts from my reports on this subject will soon appear in *Karsten's Archiv.*

With the occurrence of inflammable pit-gases in *Gerhard's* level you are acquainted from personal observation. It issues from a fissure in the coal-formation sandstone, and burns with a flame from 12 to 15 inches in height, and having a yellow colour above, but a blue one below. As this gas does not exercise the smallest pressure, or, in other words, flows out with the pressure of the atmospheric air, I was obliged to adopt a peculiar mode of collecting it. This circumstance likewise proves that the fissures or the gas-canals have numerous ramifications; when, therefore, the evolution of gas encounters the slightest obstacle, it is immediately interrupted, and the gas seeks another exit. The relatively higher temperature of this gas is remarkable. You may well believe that I did not neglect this subject; for the thermometer is, of a truth, a most valuable instrument for assisting us to answer many geological questions. I found the temperature of the gas to be 10°.55 R. (55°.74 F.), while that of a bore 8 inches deep, in the neighbouring rock, was 10°.1 R. (54°.73 F.) Assuming that the gas brings along with it the true temperature of the place where it is formed, that the mean temperature of the soil of the external crust of the earth at Saarbrück is 7°.75 R. (49°.44 F.), and that the increase of temperature towards the interior of the globe is 1° R. (2°.25 F.) for every 115 feet; the gas must have come from a depth of 322 feet. The point where the gas is evolved is 210 feet under the earth's surface, so that, according to these data, the gas must have come from a depth of 112 feet under the level. It may with probability be assumed, that the gas is exposed during its progress to cooling influences, inasmuch as it flows through colder strata, and encounters colder waters. Its original temperature may, therefore, be fairly estimated at more than 10°.55 R. (55°.74 F.), and must hence come from a greater depth.

The inflammable pit-gas of the old level of *Wellesweiler* presents, in reference to its pressure, quite a different relatio . The blower was cut out forty or fifty years ago, and was fur-

nished with a copper funnel and lengthened tube in 1816–17. It occurs in a principal fissure in slate-clay, at the bottom of the level. At some distance from the blower a bore of 119 feet has been made under the floor of the level, and a coal-bed of 70 to 80 inches in breadth has been penetrated at a depth of 42 to 49 feet. The occurrence of this gas, that is, its evolution from the water covering the floor of the shaft, allows us to conclude, that it must flow out with a pressure greater than that of the atmospheric air. There is no doubt, indeed, that all gas-canals, however complicated their ramifications may be, must be restrained by means of water throughout the whole rocky mass up to the floor of the level. It was, therefore, likewise possible that this gas should be collected by the usual pneumatic method. It overcame the pressure of a column of water 3 inches in height.

The difference in the flowing out of this gas and that of *Gerhard's* level, is easily explained. The evolution of the latter takes place about seven feet above the floor of the level, and the gas is therefore not hemmed in by the pit water. The fissure, doubtless, extends far towards the surface, and is probably branched there. When, therefore, the flowing out in the level encounters the slightest opposition, it turns aside elsewhere, where no hindrance stands in the way. The temperature of the gas in *Wellesweil* level was 10°.05 R. (54°.61 F.), and that of a bore in the surrounding rock, eight inches deep, was 8°.7 R. (51°.57 F.) According to the data stated above, this gas must have come from a depth of about 155 feet. The same remarks which were made in regard to the gas of *Gerhard's* level, apply likewise to this case.

The quantity of gas evolved in *Wellesweil* level amounts in twenty-four hours to at least eighteen cubic feet. I was unable to measure the quantity evolved in *Gerhard's* level, as it could not be collected in a pneumatic trough; by a comparison, however, of the size of its flame with that of the other, it must amount to at least twenty times as much, that is, to about 360 cubic feet in twenty-four hours. If now we take into consideration the innumerable unnoticeable evolutions besides that out of this blower, we can form an idea of the extent of the process of formation of the gas.

By what process is it possible that these inflammable gases can be produced in the interior of the earth? The author of the "*Wärmelehre*" might easily be led to the hypothesis of explaining its formation at the expense of the internal heat of the globe. Were we, however, to assume that it is produced in the same manner as the gas used for purposes of illumination, viz. by dry distillation from coal, we must take for granted that the coal-formation extends downwards to a depth where a red heat exists. But I do not for a moment doubt that such an assumption would at once be contradicted by geology. I would likewise add, that such a supposition is impossible on chemical grounds. Thus, it is known that all inflammable gases, which are obtained by dry distillation from coal and other combinations of carburetted hydrogen, contain non-permanent inflammable gases (vapours) which are absorbed by sulphuric acid. I have not, however, been able to find a trace of such vapours in either of the two pit gases analyzed. I believe further, that from all the observations hitherto made, I am entitled to assume that all inflammable gases obtained by dry distillation contain carbonic oxide gas; but careful and repeated trials for this gas have afforded only negative results. On the other hand, the resemblance in composition of the pit-gas examined by me, and the so-termed marsh-gas, renders it extremely probable, I might even say certain, that both have the same origin.

I found in pit-gases carburetted hydrogen as the chief component, and carbonic acid and nitrogen in small quantity, precisely as in marsh-gas. As, therefore, marsh-gas is evolved by a process of decomposition from organic debris, so also may inflammable pit-gas be produced by the same process from coal. In such a case the presence of water would be a *conditio sine qua non*, and many phenomena point to the view that exhalations of inflammable gases only occur where water comes in contact with coal. The evolution of inflammable gas from coal might, therefore, be a product of the continued carbonizing process of the original vegetable fibres in the moist way; a view to which Karsten has already adverted in his investigations regarding the carbonaceous substances of the mineral kingdom (Berlin 1826, p. 231). That, otherwise, the internal heat

of the earth plays an important part in this process, if coal strata reach to depths where a distinctly increased temperature prevails, is not to be doubted. Nay, it is known that the evolution of marsh-gas goes on better in the warm season of the year than in the cold, and that it is by these exhalations under the tropics that the air is infected in such a terrible manner.

I was anxious to have opportunities of examining all exhalations of inflammable gases, such as those of *Baku* on the Caspian Sea, to ascertain if they also contain no vapours which are absorbable by sulphurous acid, and no carbonic acid gas. These, however, are *pia desiderata*. The examination of the eternal fire of *Baku*, as it is called, would be of especial interest, as these evolutions of gas undoubtedly stand in connection with the springs of mineral-oil. If the mineral-oil, as has been long supposed, be a product of subterranean distillation from coal, the inflammable gases at *Baku* are most probably the result of the same process. In this case, however, these gases would contain the vapours which are condensed by sulphuric acid, and carbonic oxide gas. Without anticipating the result of such an investigation, I would remark, that many reasons might be adduced against that hypothesis, as I shall, on another occasion, endeavour to prove from observations and experiments. The assumption is, at least, extremely improbable, that such subterranean processes of distillation are still going on. It may be, then, that the central heat has, by volcanic action, reached the region of the secondary strata where there are very numerous organic remains. If a volcano were to break out anywhere in such strata, or if lavas or whole hills of melted matter were to rise through beds of coal, it cannot for a moment be doubted, that the same products would make their appearance as in our gas-works.

These considerations lead us of themselves back to those epochs, when Plutonic masses burst through all formations from greywacke to the tertiary deposits, and came in contact in all ways with organic remains. If the changes which common and brown coal have undergone in the vicinity of Plutonic masses, and of which you have so fully treated in your "*Basalt-Gebilde*," are just so many proofs of the fiery liquid

condition of these erupted masses, it is just as certain and necessary a consequence, that at that time dry processes of distillation must have gone forward on a very large scale. In those periods, therefore, inflammable gases, which were of the same composition as our artificially-prepared coal-gas, were evolved from the earth, and diffused through the atmosphere. If these exhalations continued as the Plutonic masses became hardened, and consequently became fissured, these gases would penetrate through these fissures; and they must then have been decomposed in the same manner as we artificially decompose carburetted hydrogen, when we conduct it through a red hot tube. The favourable circumstances for this occurrence existed, more especially, when Plutonic masses covered coal-strata. I have, in the course of my investigations, several times decomposed inflammable pit-gas, during its passage through a red-hot porcelain tube, and I was surprised by the extremely beautiful metallic lustre of the coal deposited in the tube. It bore a remarkable resemblance to graphite. The thought naturally occurred, if many graphites, which occur in veins and strings in granite, gneiss, porphyry, &c., have not a similar origin.

Empyreumatic oils, the liquid products of dry distillation, may partly be decomposed by the further action of the hot plutonic masses, and partly distributed through the neighbouring strata. It is a probable supposition, that many of the mineral oils which at present flow sparingly, or proceed as it were from water, have an origin of this kind. That a portion of these products of distillation even entered the Plutonic masses themselves, seems to be proved by the bitumen found by Knox in basaltic rocks. In this respect, it is particularly worthy of observation, that the wacke of *Inmarsoall*, on the coast of *Disco* Island, which occurs associated with basaltic tuffa and brown coal, contains no less than 19.4 per cent. of bitumen.

It cannot surprise us if we find bitumen also in the Plutonic masses which have burst through the transition-rocks, for the latter contain organic remains which would be decomposed by contact with the former. Some years ago, while ig-

niting basalt from *Unkel*, and other localities, in a glass furnace, in order to ascertain if any carbonic acid would be evolved, I noticed the evolution of an inflammable gas.* At that time I was much surprised by the occurrence; but now it no longer appears strange, when regarded as a consequence of the presence of bitumen. It is likewise easily understood, that, when those strata nearest the earth's surface, which have been broken through by Plutonic masses, are poor in organic remains, the bitumen of these masses may have been derived from coal strata lying at a greater depth. It would, therefore, not be very remarkable were we to find bitumen in the granite which had burst through the chalk.

In your "*Basalt Gebilde*" (Part 2d, p. 471), you state, as a fact connected with the physical history of the baths of Boll,† that, during the burning that took place there between 1633 and 1674, mineral-oil flowed in such quantity from the slate, that it was collected and sold. It is very likely that evolutions of inflammable gas occurred at the same time. No empyreumatic oil makes its appearance in what is called the burning mountain of *Duttweiler*. The fumaroles from the fissures of the clay-slate have no burning smell, and no smoke is to be observed ascending from them. This circumstance, when I first visited the locality about a year and a half ago, excited doubts in my mind as to whether there really was a coal-bed still in a state of ignition. At my request, Mr Sello, mining counsellor, and director of the mining establishment at Saarbruck, had the goodness to cause a bore to be made in the direction of the bed, and to a depth of 47 feet. At my second visit to the burning mountain, during the last autumn, I made observations on the temperature of the bore. Directly under the earth's surface, I found a temperature of 54° R. (153°.50 F.), and at a depth of five feet 69° R. (187°.25 F.); and this temperature remained the same at the deepest part of the bore. I left a copper vessel filled with oil for a

* See my "*Wärmelehre*," p. 316.

† A watering-place in Würtemberg, situated at the base of the *Rauhe Alp*.—EDIT.

night at the deepest part of the bore; which thus had sufficient time to acquire the temperature of the spot. Next morning, on drawing it rapidly up to the surface, the temperature was 72° R. (194° F.). Watery vapour issued from the bore; but there was no empyreumatic smell perceptible. Mr Sello has given me hopes that he will cause a second bore to be made on the hanging side, in order to reach the highly inclined stratum, so far as it may still be burning at a greater depth. I must say, that the above-mentioned negative facts lead me to suppose that this second bore also will encounter no burning stratum; but, at all events, it is of great interest to attain certainty on the subject.

During my two visits to the burning mountain, I neglected to make a simple experiment which would have determined decisively if the bed is still in a state of ignition, viz. to ascertain if the gases which issue from the fissures of the slate-clay are inflammable or not. In order to supply this omission, I lately begged my former pupil, Dr Jordan of Saarbrück, to make the experiment. This gentleman, who is a zealous and successful cultivator of the natural sciences, most kindly acceded to my request, and has communicated the following result:—"I have carried on my investigations with the greatest care in all the considerable fissures of the burning mountain, and have employed a Davy lamp, an ordinary mining light, and a wax taper. *There has been in no instance an inflammation of the air, in no instance an enlargement of the flame, and in no instance an explosion.* The light was soon extinguished, most speedily in the western fissures, which are lined with crystals of sulphur, and from which sulphurous acid and watery vapour issued."

After this result, one can hardly believe here in a really burning bed. Whence, then, comes the considerable heat which presents itself over a great extent? Towards the east, at a distance of 800 feet from the edge of the basin in which are the numerous *fumaroles*, I found at a depth of one foot a temperature of 14° (63°.5 F.); while, away from the burning mountain, the temperature of the ground was 12° (59° F.). At many places nearer the basin, the temperature was from

17° to 22° R. (70°.25 to 81°.50 F.). In a pit lying not far to the north, sunk in a newer stratum, I found the high temperature of 30° R. (99°.50 F.). Nearer to the reported burning bed, the temperature was probably 40° R. (122° F.) It was not possible for me to advance farther. I have hitherto omitted to measure, on the plans of the Mining Office, how far that place is distant from the bed which is supposed to be ignited. The superintendent who conducted the formation of the bore, told me that it was carried four feet into the coal, and that here the borer fell down one foot. He gave me some specimens which had been taken thence, and they were completely converted into coke. Mr Sello informed me, that in the old plans of the mines, several places are indicated by flames, to which our forefathers reached while approaching the burning strata, but which they were obliged to forsake and stop up. There cannot, therefore, be a doubt that at an early period the stratum really was on fire, and probably at that time the products of a dry distillation also made their appearance. From the reasons, already stated, however, I must doubt their still being in a state of ignition. Two causes may be conceived, which caused the high temperature that is still observable; either the rocks of the coal-formation preserve the high temperature caused by the actual burning of the stratum, or the oxidation goes on, which was formerly commenced, of the sulphur in the alum-slate. Streams of lava prove that a rocky mass, enclosed in a circular manner, and surrounded by bad conductors, which formerly acquired a very high temperature by means of the burning of a stratum, retain their original heat for a long time. I have treated of this subject in my "*Wärmelehre*," p. 493, &c. The greatest loss of heat to which the burning mountain is exposed, is from the quantity carried away by the water which penetrates into the interior, and which becomes heated, and then issues through fissures in the form of vapour. The quantity of such vapour is very considerable. In wet weather, when the vapours cannot so well escape into the atmosphere, the whole basin of the burning mountain is enveloped in mist. The atmospheric air, which probably flows in through the old deserted mining works, also carries off much warmth. I found two fissures in slate-

clay, from which no vapours issued, but merely hot air at a temperature of from 120° to 126° R. (302° to 315° F.)

The second view of the subject, viz. that the oxidation of the sulphur in the alum-slate was set agoing by the original conflagration, and is still continued, is supported by the facts, that sulphur is still sublimed, and is found in beautiful crystals on the sides of the fissures; and that the watery vapours have an acidulous, aluminous smell, like that emitted by alum-slate furnaces, and redden litmus paper.

The sal-ammoniac, which occurs as a sublimate in some of the fissures, deserves particular attention. Its formation is easily understood, when we take into account the nitrogen contained in the coal; but it is remarkable that, in the sal-ammoniac fissures, no burning smell is perceptible,—a fact which leads us to suppose the simultaneous evolution of inflammable gas. I wish to be understood to mean, that though no actual conflagration takes place in the stratum, yet the coals which occur at some distance from the former burning, can be subjected to a distillation by means of the heat which prevails in the whole rocky mass. Now, as ammoniacal salts are produced in our gas-works, and are formed at the same time as the other products of distillation; so it might be expected that this should happen likewise in burning rocks. But here the sal-ammoniac appears without the other products of dry distillation; and this is a proof that other relations must, in this case, exist.

In reference to the inflammation of the stratum, which is said to have taken place 178 years ago, it is to be remarked, that the whole coal tract of the valley of *Sulzbach*, to the Bavarian frontier, is much less inclined to inflammation than the portion lying further to the north. On the contrary, the coal there is very rich in bitumen, and at the same time very easily ignited. But, on that account, it is particularly suited to conversion into coke. The alum-slate also there has no tendency to spontaneous combustion.

By combining the phenomena presented by the burning mountain, with the previous views on the evolution of inflammable gas, and on the formation of mineral-oil, I only wished to shew, that periods can occur in coal conflagrations, in which

neither inflammable gases nor empyreumatic oils make their appearance. Whether both presented themselves in earlier times, is not known to me. Subterranean conflagrations, without the evolution of inflammable gases, can hardly be imagined; for the heat produced by a portion of a burning stratum will cause another portion of the same stratum to be exposed to a dry distillation. But the liquid products of this distillation, especially if the process occurs at some depth, can be taken up by the nearest beds; such as beds of sandstone or slate-clay, nay even of coal itself, which has not been reached by the fire; and it can be accumulated in them in the form of bitumen.

Description of a Polyphotal Lamp, and Reflector of Single Curvature, employed in Steam-vessels, Canal-boats, &c. By John Scott Russell, A.M., F.R.S.E., V.P.S.A.*

(Communicated by the Society of Arts.)

Several years ago, I was applied to by Mr Ellis, the Secretary of the Union Canal, to assist in some contrivances for expediting canal transport, and adding to its security. Amongst other things, this lamp and reflector resulted from this application. It has been in constant use ever since; it is now used also on the Forth and Clyde Canal, and is about to be introduced in steam-vessels. It is also capable of being used with advantage in many other situations, where concentration is not required.

The view with which this lamp was constructed was that of throwing light forward before the boat upon all the objects in a certain range, and to a great distance, so as to illuminate the banks of the canal, to throw light upon any vessel coming in the opposite direction,—to throw a light on the horse's path and on the arches of the bridges, so that the horses, their rider, and the helmsman, might clearly see and avoid them. All this the lamp has, for four or five years, been successfully accomplishing. It is, besides, not expensive, and is kept in order by the common boatmen without difficulty.

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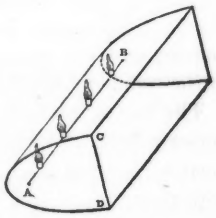
* Read before the Society of Arts for Scotland, November 13. 1839.

The lamp is polyphotal, or allows several lights or burners to be used simultaneously in the same reflector. The reflector being of single curvature, is easily made, and at little expense.

The construction of the reflector may be easily understood by conceiving a thin sheet of silver, strengthened by being united to a copper-plate, to be bent round in the middle, so as to coincide with a parabola, and form a species of conoid or surface, whose section, at right angles to the plane of the parabola, is rectilineal.

In the focal transverse axis of this figure, that is in the line joining the foci of the parabolas, at the two ends of this figure, are placed any number of lights, as in the lamp exhibited.



The figure which is thus formed has this property, that a plane vertical section through any point in the transverse focal axis A B, is a parabola whose focal distance varies with the angle of divergence. The new focus may be found by the formula: $f^1 = f^2 \cos \theta$, f being the direct focus of the primary parabola, and f^1 the focus of the parabola inclined at the angle θ.

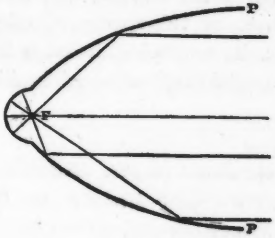
For the divergent rays, therefore, it follows that, so long as the radius of the flame does not exceed the difference between f and f^1 in the formula, the parallelism of the rays in a vertical plane will be perfect for a certain part of the flame.

I have taken, of course, for granted, as in the case before us, that lateral divergence is wanted, and that vertical divergence alone would be considered a loss; or, in other words, that light is wanted to spread forwards, on both sides, but not upwards or downwards.

The ends of the lamp are formed in the same way as the top and bottom, but this is perhaps unnecessary where horizontal divergence is of no consequence, as they might be plain surfaces without much loss.

It is of consequence, in the case of the lamp exhibited, to have the focus of the reflector short; this would bring the

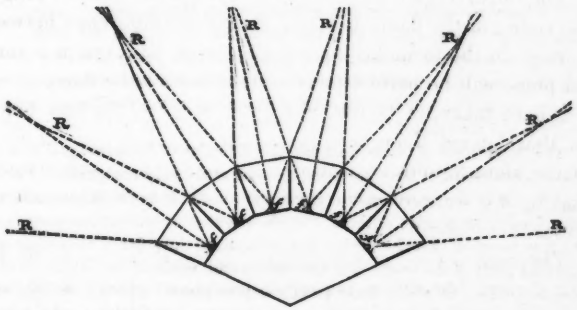
burners too close to the back, and as light is required for dispersion in the vicinity, the back is cylindrical, and concentric with the transverse focal axis A B. The effect of this is seen in the annexed diagram, where F is the focus of the parabola



and the centre of the attached semicircle, whose radius is equal to the semiparameter.

The use of such a lamp in a steam-vessel, is to shew an object a-head of the vessel, and to assist them in navigating narrow channels or rivers at night. The canal-boats go now with these lamps, in a dark night, at the rate of ten miles an hour, as safely as in the day.

The same principle is applicable to a pier-light, or a directing light, which extends through a part only of the horizon, so as to render lenses inapplicable. This arrangement is shewn in the following drawing:—



The lines *f* R, *f* R, &c., give the rays divergent in every direction horizontally, and only rendered parallel in the vertical planes, through each of the foci *f f f*. The zone of light

would thus be complete in every direction, without the dark intervals produced by reflectors of double curvature, which only emit one large parallel beam.

It is, however, to be remembered that, when it is required to carry a parallel beam of light only in a single direction, and to a great distance, this species of reflector and lamp can be of no service, its use being to carry forward a powerful light over a horizontal angle of considerable extent.

Presentation of the Royal Society Medals to Dr M. Barry and Mr Ivory, and the Copley Medal to Mr Brown. By the President, Lord Northampton.

I have the honour, Gentlemen, to inform you that the Council of the Royal Society have, by an unanimous decision, awarded the Royal Medals to Dr Martin Barry and Mr Ivory, and the Copley Medal for the year to Mr Robert Brown; and I shall now beg leave to address myself to those three Gentlemen.

Dr Barry—It gives me sincere pleasure to bestow this medal on a gentleman who has so well deserved it, by researches in a difficult and important portion of animal physiology.* Your merits have been appreciated by men much more capa-

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* These researches are the subject of Dr Martin Barry's papers "On Embryology," communicated to the Royal Society in 1838 and 1839.

In these memoirs the author has brought to light many new and interesting facts, and has repeated and confirmed previous observations regarding the nature, formation, and development of the ovum in the vertebrata, and especially in the mammalia.

The importance of the subject and the difficulty of its investigation, render the establishment of facts previously known extremely acceptable to physiologists. But the novel matter contained in Dr Barry's Memoirs forms a considerable proportion of them. Without entering into unnecessary detail, we may mention that the author has determined the order of formation of the different parts of the ovum, and the nature and mode of development of the vesicle (ovisac), in which these processes take place. He has, in like manner, discovered the nature and traced the development of the so-called disc of M. Baer, and has detected in it the mechanism which mainly regulates the transit of the ovum into the Fallopian tube. The second series of Dr Barry's observations makes known the changes which the ovum undergoes in its

ble of understanding the subject than I can pretend to be—by men selected by the Council of the Royal Society for their physiological science, who have felt the great value of the discoveries you have made by accurate and diligent research, aided by the skilful use of the microscope. I trust that the award of this medal will encourage you to persevere in the same course, and that future discoveries may add to your reputation and to that of the important profession to which you belong.

Mr Ivory—it is not the first time that you have been addressed from this chair, and it gives me great satisfaction to follow the steps of my predecessors Sir Joseph Banks and Sir H. Davy, by again bestowing a medal on one who is an honour to the Royal Society, and pre-eminently distinguished for his mathematical attainments. The labours of your life are too well known to the scientific world to require any eulogium from me, and I consider that, in this tribute to your paper on astronomical refraction, we are rather doing an honour to ourselves than to you.

Mr Brown—in conferring the Copley Medal on you for your valuable discoveries on vegetable impregnations,* I am quite sure that the voice of scientific Europe will respond to the decision of the Council of the Royal Society. The Académie des Sciences has already pronounced on your merits, as also on those of Mr Ivory, by electing you, as well as that gentleman, to a seat among their foreign members; and the Univer-

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passage through the Fallopian tube; the earliest and most interesting stages of development being for the first time described in this memoir.

The value of his very laborious and extensive series of minute observations is greatly enhanced by the clearness and method with which the results are given, and by the comparisons, which the author's intimate acquaintance with this branch of physiological literature has enabled him to institute, between his own observations and those of his predecessors in the same branch of inquiry.

* The following are the discoveries referred to, viz. the organization of the vegetable ovule, immediately before fecundation (published in 1826); and the direct action of the pollen, manifested by the contact established between it and that point of the ovulum where the embryo subsequently first becomes visible, and published in papers, in the years 1832 and 1833, and communicated to the Linnean Society.

sity of Oxford has also, by an honorary degree, given you a similar testimonial.* That you are one of our fellows, is to myself a circumstance peculiarly agreeable, as it must be to the whole body over whom I have the honour to preside. Your discoveries in the particular botanical question, for which I have to give you the Copley Medal, are so important, not only in a botanical, but also in a general scientific point of view, by shewing the close analogies of animal and vegetable life, that the Committee of Zoology have felt it as much their province as that of the Committee of Botany, to recommend that the Copley Medal should be bestowed upon you; and the Council have come to an unanimous resolution to give it, though at the same time other gentlemen were recommended by other scientific committees, with whom even an unsuccessful rivalry would be no mean praise.

I hope, Mr Brown, that you may long enjoy life and leisure to pursue researches so valuable to science, and so honourable to the country of which you are a native.

Fossil Organic Remains in Transition-Rocks in the Great Southern High Land of Scotland.

Hitherto, comparatively few organic remains have been detected imbedded in the rocks of the transition-class in the south of Scotland. The well-known beds of transition-limestone near to the Crook Inn, on the road from Edinburgh to Moffat, afford fossil shells and corals; and thin layers and disseminated portions of glance-coal (Anthracite) occur in the greywacke series of Queensberry Hill, in the upper part of Dumfriesshire. *Graptolites*, which, on a general view, so much resemble plants, but belong to the Radiaria of zoologists, have been long known as fossils of our greywacke system; even in this neighbourhood, as at Grieston Slate Quarry, in a hill on the south bank of the river Tweed, about five miles from Peebles, and one and a half from Innerleithen, one of our pu-

* On occasion of the Meeting of the British Association at Edinburgh, in 1834, the University of that City conferred on Mr Brown the honorary degree of LL.D., and the Lord Provost and Council at the same time presented their illustrious countryman with the freedom of the Scottish Capital.—Edit.

pils, Mr James Nicol of Innerleithen, procured from the roofing slate fine specimens of Graptolites. About two miles farther down the Tweed, in the Cadon Bank Hill, glance-coal has lately been discovered associated with greywacke and transition clay-slate. Lord Traquair, the proprietor of the district, is endeavouring, by mining operations, to ascertain if the coal occurs in such quantity as to render it available in an economical point of view. Mr Nicol, who has examined this new locality of glance-coal, informs us that the beds of coal, which appear to follow the direction of the strata, do not exceed, so far as hitherto explored, a few inches in thickness, and in some places exhibit a fibrous structure. Mr Kemp, lecturer on chemistry, finds the coal to be nearly pure carbon.

Proceedings of the Wernerian Natural History Society.

(*Continued from* Vol. xxvi. p. 429.)

March 23. 1839.—Dr Traill, formerly V. P. in the Chair. Mr Robert J. Hay Cunningham read a paper on the Silurian System of Murchison, and illustrated the same by coloured sections and numerous specimens. Dr Traill shewed to the meeting some of the dark-coloured Sand found among Teas imported from China last season, and which he had found to be manganesian iron-sand, apparently sprinkled over the leaves, for the purpose of increasing the weight of teas. The Doctor also exhibited portions of a large Feline Animal found in Cefn Cave, Wales.

April 6.—Professor Jameson, P. in the Chair. The Assistant-Secretary read a paper by Dr Haworth on the Formation of Calcareous and Magnesian Rocks, viewed geologically and chemically.

April 20.—Professor Jameson, P. in the Chair. The Assistant-Secretary read a paper by Dr Hamilton Kinglake, entitled, Observations on Mr Crosse's Electrical Experiments. The President laid before the meeting the list of subjects for Honorary Prizes, which was published in vol. xxvii. p. 206, of this Journal.

The opening of the Thirty-third Session of this Society took place on 23d November 1839, William Copland, Esq. V. P. in the Chair. The principal business was the election of office-bearers for 1840, when the following were chosen:—

President.

Robert Jameson, Esq. F.R.SS. L. & E., Professor of Natural History in the University of Edinburgh.

Vice-Presidents.

R. K. Greville, Esq. LL.D. — Dr Walter Adam.
John Sligo, Esq. F.R.S.E. — Dr T. S. Traill, F.R.S.E.

Secretary.—Dr Pat. Neill, F.R.S.E. *Librarian.*—J. Wilson, Esq. F.R.S.E.
Assist. Sec.—T. J. Torrie, Esq. F.R.S.E. *Painter.*—P. Syme, Esq.
Treasurer.—A. G. Ellis, Esq. *Assist. Paint.*—W. H. Townsend, Esq.

Council.

R. J. Hay Cunningham, Esq. — Sir William Newbigging, F.R.S.E
W. A. Cadell, Esq. F.R.SS. L. & E. — David Falconar, Esq.
Dr R. Hamilton, F.R.S.E. — Dr Robert Paterson.
Dr Robert Graham, F.R.S.E. — Edward Forbes, Esq.

NEW PUBLICATIONS.

1. *Journal of the Asiatic Society of Bengal.* Edited by the Acting Secretaries.

The departure of Mr James Princep, the former Editor, from India, as stated in our last number, has placed the editorial duties of this valuable periodical in the hands of the Acting Secretaries of the Society. The first and second numbers of this new series have reached us. The *First or January Number*, 1839, contains the following scientific articles:—1. *On the Distribution of European Birds* (*particularly in reference to the views of Mr Swainson*), by William Jameson, Esq. Bengal Medical Service. 2. *On a new Genus of the Fissirostral Tribe:* By B. H. Hodgson, Esq. Catamandu (with a plate). 3. *On the Egyptian System of Artificial Hatching:* By Don Sinbaldo Demas (with a plate). 4. *Dr Burke's Report on the Value of Life among the Officers and Men in Her Majesty's Troops in India.* 5. *Observations on the Burmese and Munipoor Varnish Tree:* By N. Wallich, Esq. M.D. (with a plate). 6. *Meteorological Table kept at the Assay Office, Calcutta, for the month of January* 1839.—The number of February 1839 contains the following scientific papers:—1. Mr Hodgson *on Cuculus.* 2. *Report on the Coal and Iron Mines of Talcheer and Ungool, &c. &c.:* By Mr M. Kittoe. 3. *On the Detection of Arsenical Poisons, &c. &c.:* By W. B. O'Shaughnessy, Esq. M. D. 4. *Proceedings of the Asiatic Society.* 5. *Meteorological Table for month of February.*

2. *Madras Journal of Literature and Science.* Published under the auspices of the Madras Literary Society, and Auxiliary Royal Asiatic Society. Edited by the Secretaries to the Asiatic Department.

This Journal, like that of the Asiatic Society of Bengal, is highly creditable to the science and literature of our countrymen in the East. From the valuable information it communicates to the public, we doubt not that its continuance is amply secured. In future, as the numbers reach us, we shall communicate to our readers their general contents. The first number of the present year 1839—including the months from January to March, the only one which has reached Scotland—contains, besides other articles, the following:—1. *Geology of Bangalore, and of some other portions of Mysore:* By JOHN CLARK, Esq. M. D., Assistant-Surgeon, 13th Light Dragoons. 2. *Remarks on Gambogia Gutta, Linn.; Stalagmitis Gambogioides, Murray; and on Laurus Cassia, Linn.:* By ROBERT WIGHT, Esq. M. D. 3. *Report upon the Run of the Sea, and Set of the Tides, at Madras, during the NE. Monsoon:* By T. G. TAYLOR, Esq. Honourable East India Company's Astronomer. 4. *A Remarkable Appearance in the Indian Seas,* in a Letter from Lieut. DAWSON. 5. *Proceedings of the Madras Asiatic Society.* 6. *Horary Meteorological Observations made at the Madras Observatory:* By T. G. TAYLOR, Esq. H. E. I. C.'s Astronomer.

3. *Narrative of the Surveying Voyages of H. M. Ships Adventure and Beagle, between the years* 1826 *and* 1836, *describing their Examination of the Southern Shores of South America, and the Beagle's Circumnavigation of the Globe.* Vol. III. Also entitled "*Journal and Remarks,* 1832–1836:" By CHARLES DARWIN, Esq. M.A., Sec. Geol. Soc. London, H. Colburn. 8vo, pp. 615.

This delightful volume displays a very enviable exuberance of talent. It much resembles the classical work of Humboldt, and therefore is distinguished by its beautiful views of nature, and variety of interesting details in all the departments of natural history. We look forward with pleasure to its continuance.

4. *Mineralogisch-geognostiche Beschreibung der Umgegend von Schriesheim* Von GUSTAV LEONHARD. Stuttgard, 1839.

This interesting tract contains much curious information in regard to the Porphyry of the Bergstrasse. It is very creditable to its young author, son of the celebrated Professor Leonhard of Heidelberg.

List of Patents granted for Scotland from 9th October to 21st December 1839.

1. To PETER LOMAX of Bolton-le-Moors, in the county of Lancaster, weaver, for an invention of "certain improvements in looms for weaving."—9th October 1839.

2. To JOSEPH GARNETT of Haslingden, in the county of Lancaster, dyer, in consequence of a communication from a foreigner residing abroad, for an invention of "improvements in machinery or apparatus for carding, drawing, roving, and spinning, cotton, flax, wool, and other fibrous materials."—9th October 1839.

3. To JOSEPH DAVIES of Nelson Square, in the county of Surrey, gentleman, for an invention of "a composition for protecting wood from flame."—9th October 1839.

4. To WILKINSON STEELE and PATRICK SANDERSON STEELE, manufacturing and furnishing ironmongers, George Street, Edinburgh, for an invention of "improvements in kitchen-ranges for culinary purposes, and apparatus for raising the temperature of water for baths and other uses."—18th October 1839.

5. To WILLIAM EDMONDSON and JAMES EDMONDSON, both of Manchester, in the county of Lancaster, engineers, for an invention of "certain improvements in the machinery or apparatus for the manufacture of wood-screws and screw-bolts."—19th October 1839.

6. To ROBERT STEWART of North Woodside, near Glasgow, ironsmith, for an invention of "an improved crane for raising stones, or other heavy substances, from quarries or other works."—22d October 1839.

7. To SAMUEL HALL of Basford, in the county of Nottingham, engineer, for an invention of "improvements in steam-engines, and in propelling."—22d October 1839.

8. To ALEXANDER BORLAND of Paisley, in the county of Renfrew, in Scotland, accountant, for an invention of "a machine for measuring water and other liquids, and registering the quantity thereof."—26th October 1839.

9. To JAMES SMITH of Deanston Works, in the parish of Kilmadock, and county of Perth, cotton-spinner, for an invention of "certain improvements applicable to canal navigation."—26th October 1839.

10. To GEORGE CHAPMAN of Whitby, in the county of York, engineer, for an invention of "certain improvements in steam-engines."—26th October 1839.

11. To SAMUEL WILKS, of Darsleton, in the county of Stafford, iron-founder, for an invention of "improvements in boxes and pins, or screws, for vices and presses."—28th October 1839.

12. To THOMAS NICHOLAS RAPER of Bridge Street, Blackfriars, in the city of London, for an invention of "improvements in rendering fabrics and leather waterproof."—28th October 1839.

13. To ROBERT EDWARD MORRICE of King William Street, in the city of London, gentleman, in consequence of a communication from a certain foreigner residing abroad, for an invention of "improvements in the manufacture of boots and shoes, and coverings for the legs."—28th October 1839.

14. To JAMES SMITH of Deanston Works, in the parish of Kilmadock, and county of Perth, cotton-spinner, for an invention of "a self-acting temple, applicable to looms for working fabrics, whether moved by hand or power."—1st November 1839.

XUM

15. To JAMES YATES of Effingham Works, Rotherham, in the county of York, ironfounder and earthenware-manufacturer, for an invention of "certain improvements in the construction of cupola or blast furnaces for melting metals, which improvements are also applicable to furnaces or fireplaces for other purposes."—1st November 1839.

16. To JOHN BARNETT HUMPHREYS of Southampton, civil-engineer, for in invention of "certain improvements in shipping generally, and in steam-vessels; and in particular, some of which improvements being individually novel, and some the result of novel applications or combinations of parts already known."—1st November 1839.

17. To JOHN GEORGE BODMER of Manchester, in the county of Lancaster, civil-engineer, for an invention of "certain improvements in machinery or apparatus for cutting, planing, turning, drilling, and rolling metals and other substances."—8th November 1839.

18. To WILLIAM NEWTON, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil-engineer, in consequence of a communication from a certain foreigner residing abroad, for an invention of "certain improvements in machinery or apparatus for making or manufacturing screws."—11th November 1839.

19. To JAMES SUTCLIFFE of Henry Street, Limerick, builder, for an invention of "certain improvements in machinery or apparatus for raising and forcing water or other fluids, and increasing the power of water upon water-wheels, and other machinery."—11th November 1839.

20. To JAMES ULRIC VAUCHER of Mount Street, Grosvenor Square, in the county of Middlesex, for an invention of "certain improvements in fire-engines and other hydraulic machines and apparatus for raising or propelling water and other fluids, some of which improvements are also applicable to steam-engines."—11th November 1839.

21. To JAMES CRAIG of Newbattle Paper-Mills, in the parish of Newbattle, and county of Edinburgh, for an invention of "an improvement or improvements in the machinery for manufacturing paper."—11th November 1839.

22. To PIERRE AUGUSTE DUCOTE of No. 70 St Martin's Lane, in the county of Middlesex, for an invention of "certain improvements in printing china, porcelain, earthenware, and other like wares; and also for printing on paper, calicoes, silks, woollens, oilcloths, leather, and other fabrics and for an improved material to be used in printing."—14th November 1839.

23. To JOHN DICKINSON of Nash Mill, in the county of Hertford, paper-maker, for an invention of "improvements in the manufacture of paper."—18th November 1839.

24. To GEORGE HANSON of Huddersfield, in the county of York, plumber and brazier, for an invention of "certain improvements in the construction of cocks or taps for drawing off liquids."—19th November 1839.

25. To WILLIAM WIESMANN of George Yard, Lombard Street, in the city of London, merchant, in consequence of a communication from a certain foreigner residing abroad, and partly by invention of his own, for "improvements in the manufacture of alum."—22d November 1839.

26. To CHARLES ANDREW CALDWELL of Audley Street, in the county of Middlesex, Esq. in consequence of a communication from a certain foreigner residing abroad, for an invention of "improvements in furnaces and apparatus for supplying the heat of fuel."—22d November 1389.

27. To FREDERICK CLARK of Chelsea, in the county of Middlesex, in consequence of a communication from a certain foreigner residing abroad, for an invention of "improvements in building ships, steam-vessels, and boats, and also in the building of canal and river barges, and lighters."—22d November 1839.

28. To THOMAS KERR, Esquire of Forecrofts, Dunse, in the county of Berwick, for an invention of "a new and improved mortar or cement for building and other purposes."—23d November 1839.

29. To WILLIAM MILLER of Clitheroe, in the county of Lancaster, engineer, for an invention of "certain improvements in grates used in steam-engine or other furnaces or fireplaces."—5th December 1839.

30. To JOHN SWINDELLS of Manchester in the county of Lancaster, manufacturing-chemist, for an invention of "certain improvements in the manufacture of Prussian blue, prussiate of potash, and prussiate of soda."—5th December 1839.

31. To FRANCOIS VONILLON of Prince's Street, Hanover Square, in the county of Middlesex, silk-mercer, in consequence of a communication from a certain foreigner residing abroad, for an invention of "improvements in the manufacture of ornamental woven fabrics."—6th December 1839.

32. To GEORGE LOWE, engineer to the Chartered Gas Company, and JOHN KIRKHAM, engineer to the Imperial Gas Company, both of London, for an invention of "improvements in the manufacture of gas for purposes of illumination."—6th December 1839.

33. To JOHN JUCKES of Shropshire, gentleman, for an invention of "improvements in furnaces and fireplaces for the better consuming of fuel."—12th December 1839.

34. To MOSES POOLE of Lincoln's Inn, in the county of Middlesex, gentleman, in consequence of a communication from a certain foreigner residing abroad, for an invention of "improvements in looms for weaving."—12th December 1839.

35. To CHARLES DOD of 21 Craven Street, Strand, in the county of Middlesex, gentleman, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "certain improvements in the construction of railways and tram-roads, and in the carriages to be used thereon and otherwise."—18th December 1339.

36. To CHARLES COWAN of Penicuick, in the county of Edinburgh, in that part of the United Kingdom called Scotland, paper-manufacturer, and ADAM RAMAGE of Valleyfield in the same county, paper-maker, for an invention of "improvements in machinery used in the manufacture of paper."—19th December 1839.

37. To WILLIAM PALMER of Satton Street, Clerkenwell, in the county of Middlesex, lamp-maker, for an invention of "improvements in lamps, and in the manufacture of candles."—21st December 1839.

38. To JOHN SUTTON of John St, Roupell Street, Lambeth, in the county of Surrey, machinist, for an invention of "improvements in obtaining power."—21st December 1839.

39. To AMBROSE BOWDEN JOHNS of Plymouth, in the county of Devon, artist, for an invention of "improvements in colouring or painting walls and other surfaces, and preparing materials used for that purpose."—21st December 1839.

40. To ANTONIO JAMES MAYER of Ashley Crescent, in the parish of St Luke, in the county of Middlesex, gentleman, for an invention of "an improved machine for cutting splints for matches."—21st December 1839.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

*On Daguerreotype.** By Andrew Fyfe, M.D., F.R.S.E., F.S.A., &c. Communicated by the Society of Arts of Scotland.

The intense degree of interest excited, a few months ago, by the accounts of the different photographic processes, has now given way to that raised by the disclosure of Daguerreotype. Though by photography beautiful representations may be obtained, yet, I believe, it is now generally allowed, that it does not come up to our expectations in giving delineations by the camera, with that degree of clearness and delicacy that was anticipated. In this country, at least, I have never seen designs taken by the camera of any value. This remark does not, however, apply to Daguerreotype. High as our expectations were raised by the first accounts of it, now that the secret is divulged, and that we have had opportunity of inspecting specimens, I would say that the accounts given of it were far from exaggerated—nay, that the delineations possess a richness and delicacy beyond what the most sanguine had anticipated.

My object in making a few remarks on this interesting process, is chiefly with the view of illustrating its *rationale*. The subject is as yet in its infancy. No doubt numerous improvements will, ere long, be suggested; and nothing, I conceive, is more likely to conduce to this than an accurate knowledge of the *rationale* of every stage of the process. It

* Delivered before the Society of Arts for Scotland on 15th January 1840.

is with this view that I have been induced to investigate it; and I trust that the results I have obtained are not devoid of interest, not only as tending to illustrate what I conceive to be the action that occurs, but also as leading to some important practical applications in the arts.

On the first announcement of the mode of conducting daguerreotype, M. Arago conjectured that the effect, ultimately brought out by the hyposulphite, might be owing to its washing off the iodine, and probably to the sulphur of the salt acting on the silver, so as to convert it into a black sulphuret which, contrasting with the white mercurial coating, gives the dark and light parts of the picture. It is evident, however, that this is not the true *rationale*, because it is stated by Arago himself, that a similar result is obtained by washing the plate with solution of sea-salt, and where no sulphuret can be produced. The action, in my opinion, is altogether different; it does not depend on the formation of compounds differing in their nature and appearance, by the agency of the hyposulphite, but on metallic surfaces being presented, by which the light is reflected differently.

The first part of the process—the polishing—must be considered as merely mechanical; and certainly the more highly resplendent the surface, the more likely we are to succeed in getting a distinct image on it. With regard to the use of the diluted acid, its action is not so easily accounted for. That it should act on the silver, and form a nitrate, as some have supposed, is not likely, when we consider its great state of dilution, and the very minute quantity employed—two or three drops being sufficient for a large plate. According to some it may wash off any copper adhering to the surface of the silver. To ascertain whether or not it acted on the metals, I raised a wall of wax-luting around a polished plate, and kept on it diluted acid (1 to 16) for some time, rubbing the plate occasionally with cotton. On testing the fluid, I could not detect the slightest trace of silver in it. Another plate was acidified and heated as recommended by Daguerre. It was then washed with distilled water; and the washings were tested for silver, with the same result.

The fluid from the plate in the preceding experiment, when

tested for copper, gave indications of its presence. It is evident, then, that the acid acts merely by washing off impurities, and thus allowing a purer surface to be afterwards exposed to the iodine.

Different opinions have been entertained with regard to the action of the iodine. Some suppose that the golden-coloured coating is merely a thin film of iodine, adhering to the surface of the silver; while others maintain that it is an *iodid* of silver. Were it the former, we should expect it to be easily rubbed off, and more particularly that it would be dissipated by heat; neither of which occurs. When heated—excluded from light—the plate still retains the golden-yellow colour. Potassa has no action on it, which it should have, were it a mere film of iodine. From these and other experiments of a similar nature, it is evident that the iodine does not merely adhere to the silver, but that it combines with it and forms an *iodid;* the colour of which depends on the time the plate has been exposed to its action.*

When the iodinized plate is kept in the camera *for the proper time*, it does not appear to be affected in the slightest degree, and it is not till after it is exposed to the mercurial vapour that the image appears. It is evident, however, that the light must exert some peculiar action on the iodid. Either the affinity between the iodine and the silver must be overcome, and the iodine be set free from the silver, or the constitution of the iodid must be altered. When the plate is kept *long* in the camera, it may be so affected as to bring out an indistinct image, a circumstance which I have occasionally observed; but this result is more easily obtained by exposing the plate to the direct rays of the sun, or even to a dull light, with an object—say an engraving—placed on it, as in the common photographic process, with paper prepared with the chlorid or phosphate of silver, in which case a distinct representation of the object on the engraving is produced—those parts exposed to the light becoming dark, while those excluded from

* When the plate is long exposed to the vapour of iodine, it presents a rich display of prismatic colours, sometimes in concentric rings, sometimes passing from side to side.

it retain their original golden hue. When the plate is long exposed, on the parts on which the light impinges, the yellow is changed to dark violet, and, by slight friction, a dark coloured powder is removed. When this is treated with water or alcohol, the fluids do not, on the addition of the usual tests, indicate the presence of uncombined iodine. The iodinized plate, after long exposure to light, so as to darken it, does not, when heated, give off vapour of iodine; nor is its surface at all changed, from which it is evident that the powder liberated from the plate, by the agency of the light on the iodid, is not free iodine. Most probably it is the iodid set free from the silver, but the quantity procured is so very minute that I have not been able to satisfy myself as to its chemical composition.

The most important question regarding this interesting subject refers to the last stages of the process—the mercurializing and washing with hyposulphite.

When the plate is removed from the camera, and without any image observed on it, and is then exposed to the vapour of mercury, the mercury evidently adheres to those parts only which have been acted on by the light. Of course, the greater the action of the light, the more is the iodid acted on, and the thicker is the mercurial coating acquired, or the more readily does the mercury adhere to the silver; thus giving different degrees of density, or a difference of surface, while those parts on which the light has not impinged still retain their golden-yellow hue.

On washing with hyposulphite, the whole of the iodid is removed, and silver and mercurial surfaces are then exposed; and it is the contrast between them, and not any particular chemical action exerted on the silver, by the hyposulphite, that produces the effect. In fact, it is a mere difference in reflection of light from a highly resplendent silver plate, and from a dull mercurialized surface; the former presenting the dark, the latter the light parts of the picture; the variety of shades, from dark to light, being brought out by a difference in the density, or by inequality in the mercurialized parts, according to the action of the light; for we can easily conceive that, the less and less the action of light, the thinner and thinner, or more imperfect, may be the mercurial coating,

XUM

and thus all the variety of shades may be given, keeping in view that the silver surface always appears dark when in contrast with the mercurial one.

That this is really the cause of the brilliant effects brought out, is, in my opinion, proved by a very simple experiment. If a plate, polished as for daguerreotype, with its highly resplendent surface, have an object placed on it—as a piece of paper cut to any form, and moistened to make it adhere, and be then exposed in the mercurializing box, the mercury being heated to the temperature recommended by Daguerre—those parts exposed become mercurialized, while those covered with the paper are not acted on. When the paper is removed, we have a representation of the object, having all the appearance of a daguerreotype, excepting that it is merely in *profile;* the silver surface, or that representing the object, being dark, the merculiarized surface being light. A similar result may be obtained on other surfaces, as tin, tinned iron, and copper, provided they be highly polished; even on glass the same effects may be produced, but in this last case it is necessary to mercurialize at a higher temperature than for daguerreotype, and the plate, after being mercurialized, must be held in a particular position, otherwise the object is not distinctly observed.

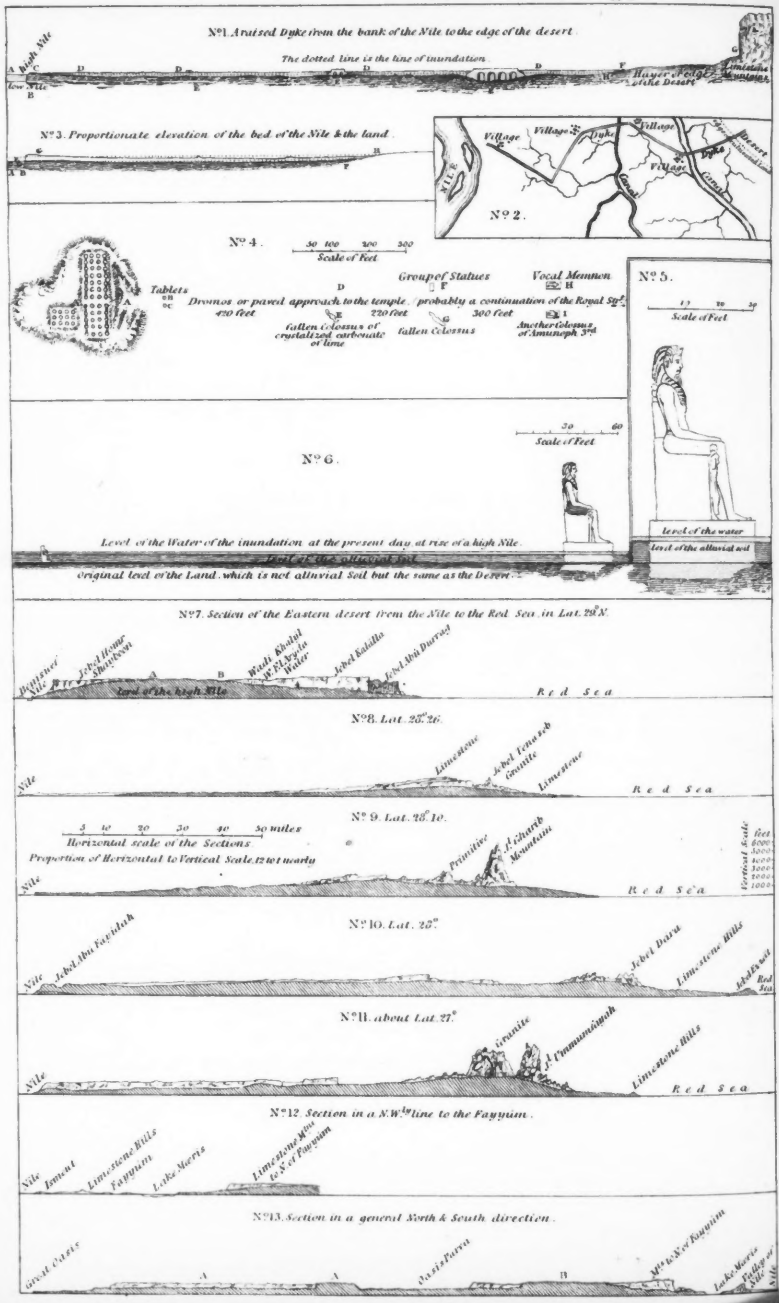
Daguerre has stated that, in Paris, the time that the plate should be kept in the camera, during favourable weather, should not exceed about three minutes; and points out the necessity of operating in the dark with the iodinized plate. Though it may appear to some that there is no absolute necessity for this precaution, I may mention, as corroborative of what Daguerre has stated, that if an iodinized plate, with an object placed on it, be exposed to direct sunshine, and *instantly* withdrawn, a distinct image will be found on it by mercurializing and washing with hyposulphite.

Daguerre has also remarked that, when inspecting the plate during the mercurializing process, the light, even from a candle, should not be allowed to flow long on it. However improbable this may appear, yet it is not less true. The light of a gas lamp affects it powerfully; and hence, by exposing a plate to a gas flame, brilliant impressions can be procured. I

have in this way succeeded in getting impressions from engravings, having all the delicacy of the original. For this purpose it is necessary to have the engraving on thin paper, placed on the plate with the engraving exposed to view, so as not to have a reverse. They are kept together by putting them into a glass frame, and then exposed near the flame either of an argand or flat burner for about twenty minutes or so, according to the light and the thickness of the paper, after which the plate is to be mercurialized and washed with hyposulphite.

I have already mentioned that when a polished plate, with an object placed on it, is mercurialized, an image is presented similar to a daguerreotype, and which, like it, is easily obliterated. It is remarkable, however, that if, instead of mercurializing at the temperature of about 160° Fahr., we raise it to about 400° Fahr., not only does the mercury present a whiter surface, but it adheres so firmly to the plate that it requires the polishing powder to remove it. This affords an easy method of producing and multiplying to any extent *profiles* on silver. The profile on paper is cut out, and, after being moistened on both sides with the tongue, or with very weak mucilage, is placed on the polished plate, and pressed down so as to make it adhere to it at all points; after which it is put into the mercurializing box, and kept exposed to the vapour of the mercury till the temperature reaches to about 400° Fahr. The lamp is then removed, and the temperature allowed to sink before taking out the plate. In conducting this process, the only precaution necessary is to take care that the paper does not separate from the silver, which it is apt to do when the heat is raised too high, or the plate is long kept in the box, in which case the image is indistinct. To prevent this, the weak mucilage is used, which is easily washed off from the silver by keeping the plate in warm water, or rubbing it softly with a sponge. Should the profile not be sufficiently sharp, the process may be repeated on the same plate, by removing the mercury with the polishing powder.

Sections to illustrate Sir J. G. Wilkinson's paper on the levels of Egypt.



*On the Nile, and the present and former Levels of Egypt.** By Sir J. Gardner Wilkinson. (With a Plate.)

The nature and character of the Nile, and the peculiar laws which govern the land of Egypt, are questions which, in all times, have been looked upon with considerable interest. Numerous conjectures were formed by ancient writers respecting the probable cause of the inundation. Some attributed it to the continued force of the Etesian or annual winds, which, blowing from the northward during that season of the year, were supposed to check the course of the stream, and to occasion it to overflow; an opinion readily refuted by Herodotus.† Others explained it by the melting of snow in the lofty mountain-ranges of Ethiopia; and some were disposed to believe, that periodical rains falling there accounted for this phenomenon.

Though the reasoning of Herodotus on the subject is not very philosophical, it is evident that he attributes the rise of the Nile to the rains which fall near its source, an opinion common, as Strabo informs us,‡ to many, and one that Homer, from his calling the river διιπετης, or "fallen from heaven," appears to have adopted. Modern discoveries have shewn the truth of this conjecture; and, as far as regards the cause and sources of the Blue River, or eastern branch, and its tributary streams, our knowledge is tolerably accurate. The White River, its sources, and the extent of its course from its head until it joins the Blue River at Khartúm, in Lat. 15° 38′, are still a desideratum; and the only part of the stream hitherto examined is a distance of thirty days' march above the junction. It is, however, to be hoped, that a native of Ethiopia, lately sent from England for the purpose, will clear up this important question, and add to our geographical knowledge by ascertaining the course and sources of the White River. That this last is the main stream, is universally allowed by every one who has visited it, from Bruce till the present day; but the

* From Journal of the Royal Geographical Society of London, vol. ix. part iii. p. 431, *et seq.*

† Herodot. ii. 20.

‡ Strabo, xvii. p. 543.

Blue River possesses a remarkable character, which connects it more closely with the inundation, and claims for it the merit of being the parent of the beneficial qualities of that river which spreads fertility throughout its course from Abyssinia to Egypt.

The White River brings no such alluvial deposit: the sandy soil of its banks is unsuited to many of the productions which flourish in the other branch; and though its additional stream, rising about the same time as the Blue River, tends to raise their combined waters over the lands they fertilize in their course northward, the Egyptian peasant has merely this debt of gratitude to acknowledge; and the prayers of a heathen husbandman might be offered to the supposed god of the Abyssinian branch for the blessings of the inundation, without his being bound in duty to propitiate the presiding deity of its western companion. The Blue River has the same general character as that observable throughout the course of the Nile. Its banks in Ethiopia and Egypt are formed of the same rich alluvial deposit brought from the mountains of Abyssinia, and the principal difference is in the greater thickness of the stratum left in the southern part of its course, in consequence of the heavier particles subsiding more quickly than those lighter ones which are carried onwards in its course to Egypt.

To give some idea of the manner in which the alluvial deposit takes place, and the changes it causes in the levels of the land, and in the bed of the river itself throughout its course, I must first observe, that the bed of the Nile and the land of Egypt (to which country I shall now confine my remarks) undergo a gradual increase of elevation, varying in different places according to circumstances, and always lessening in proportion as the river approaches the sea. This increase of elevation, in perpendicular height, is much smaller in Lower than in Upper Egypt; and, in the Delta, it diminishes still more, so that, according to an approximate calculation, the land about Elephantine, or the first cataract, in Lat. 24° 5′, has been raised nine feet in 1700 years; at Thebes, in Lat. 25° 43′, about seven feet; and at Heliopolis and Cairo, in Lat. 30°, about five feet ten inches. At Rosetta, and the mouths of the Nile, in Lat. 31° 30′, the diminution in the per-

pendicular thickness of the deposit is lessened in a much greater decreasing ratio than in the straitened valley of Central and Upper Egypt, owing to the great extent, east and west, over which the inundation spreads, and there the elevation of the land, in the same period of 1700 years, has been comparatively imperceptible. In like manner, the proportion between the increase at Elephantine and Thebes differs from that between Thebes and Heliopolis, because the breadth of the valley is greater below Thebes, and because the farther southward the more is the deposit. In one case, $1\frac{1}{2}^{\circ}$ of latitude gives a difference of about two feet; in the other (from Thebes to Heliopolis), $4\frac{1}{2}^{\circ}$ give a difference of only one foot two inches.

Those arguments used to shew the effects of the alluvial deposit in rapidly protruding the Delta into the sea, founded on the statement of Homer respecting the isle of Pharos, will not, I trust, be again brought forward, since positive facts prove the limited progress made by the Delta, from the earliest times of which any record exists, by the position of ancient cities, as Pelussium, Canopus, and others, whose sites are still in the vicinity of the sea-coast, but which, if any great protrusion of the land into the Mediterranean had actually taken place, ought at this time, after a lapse of between 3000 and 4000 years, to be far inland.

With regard to the statement of Homer,* that "the distance from the isle of Pharos to Ægyptus was as much as a vessel, with a fair wind, could perform in one day," I have shewn,† that all arguments derived from it are inadmissible, in consequence of the situation of that island and the nature of the ground on which Alexandria is built. That city stands on the rock of the Libyan desert, which is still, as it ever was, above the reach of the inundation; and the breadth of the channel, between the shore and the island of Pharos, was, in the days of Homer, and at every period, precisely the same. After the foundation of Alexandria, the island was united to the shore by an artificial dyke called the Heptastadium; but, though this connected them, it did not bring the shore one

* Odyss. Δ, 355.

* Manners and Customs of the Ancient Egyptians, vol. i. p. 7.

foot nearer to the island, nor was any alluvial deposit the cause of the channel between them being closed. And now, having shewn that the deposit of the Nile had no power to advance the shore towards the Isle of Pharos, I beg to exculpate the poet from the imputation of a gross error, which might otherwise attach to his assertion, by observing, that he uses the word Ægyptus to signify the Nile, as well as the land of Egypt, which is fully explained by Diodorus*, who says that Nileus, one of the early monarchs, transferred his name to the stream, which previously bore that of Ægyptus; and Arrian† observes that "the river, now called by the Ægyptians and others Nile, is shewn by Homer to have been named Ægyptus, when he relates that Menelaus anchored his fleet at the mouth of the Ægyptus."‡ It is, then, to the Nile, and not to the coast of Egypt, that the poet refers, when he speaks of the distance from Pharos to Ægyptus.

The opinion of Herodotus and others, that the constant elevation of the land by the alluvial deposit would eventually prevent the inundation covering the lands, has been repeated even to a late time; and some have thought, that all the predictions of famine made by the historian were on the eve of their fulfilment. The Nile, they say, formerly rose so high above the land, that Herodotus saw the villages during the inundation, like the islands in the Ægean Sea. This ceases to be the case at present, and, after some years, it will no longer inundate the country at all. But this opinion is maintained by its authors merely from their not having visited Egypt during a great rise of the river; while, from my own experience, and that of others, I can attest that the same happens at the present day as in ancient times, whenever the inundation is of a certain height; for it is well known, that, in every age, the Nile varied in its rise, and the deficiencies of one or two seasons were counterbalanced by a plentiful supply of water in another year. Writers, who held this argument and foretold such dreadful calamities to the unsuspecting inhabitants of Egypt, forgot to observe that the bed of the Nile always keeps pace with the elevation of the soil, and the proportion of wa-

* Diodor. i. 63.

† Arrian, Exped. Alex. 5 and 6. ‡ Odyss. Δ, 477, and Ξ, 257.

ter annually brought down by the river has always, and ever will be, the same; the only difference being, that it now overflows a greater extent of land, east and west, than in former times, and that the superficies of cultivable land in the broad plains of Central Egypt and the Thebaid continues to increase.

In that part of Egypt lying to the south of the Delta, the banks of the Nile are much more elevated than the land of the interior at a distance from the river, and they are seldom quite covered with water even during the highest inundations. Little, however, projects above the level of the stream, and, in some places, the peasant is obliged to keep out the water by temporary embankments. This may be accounted for partly by the continued cultivation of the banks, which, being more conveniently situated for artificial irrigation, have a constant succession of crops; for it is known that tillage has the effect of raising land, from the accumulation of decayed vegetable substances, the addition of dressing, and other causes; and the greater depression of the plain in the interior is probably, in some degree, owing to the numerous channels in that direction, and to the effect of the currents which pass over it as the water covers the land. It must, however, be confessed, that these causes are not sufficient to account for the great difference existing between the height of the bank and the land near the edge of the desert, which often varies as much as twelve and fifteen feet, as may be seen from the respective heights of the dykes at those two points. These elevated roads, the sole mode of communication by land from one village to another during the inundation, commence on a level with the bank of the river, and, as they extend to the interior, rise to so great a height above the fields, as to leave room for the construction of arches for the passage of the water, though, generally speaking, bridges are only built in those parts where ancient or modern canals have lowered the levels sufficiently to admit of them.

The general appearance of the dykes may be illustrated by No. 1. of Plate II., in which A is the surface of the Nile during the inundation; B the level of the low Nile; C the bank; DD the raised dyke; E the beds of canals, over which bridges

are built in the dyke ; and F the *Hajer* or slope of the desert, extending from the junction of the irrigated land at H to the limestone mountains, G. This section is given as if the dyke were in one straight line, E. and W. from the river; but in reality they follow a tortuous course, visiting the various towns on their way, and serving as roads, as well as an impediment to the arbitrary overflow of the inundation. The direction of a dyke, varying according to circumstances, may be represented as in No. 2. It is on a plain of about five miles in breadth. Some dykes are much more circuitous and indirect even than this, but in all cases the principal care is to place them so as to oppose the greatest force to the largest body or pressure of water, and to offer the readiest means of communication from one village to another.

I have already observed that the deposit gradually raising the bed of the river, and the proportionate elevation of the water of the inundation, tend to increase the extent of the arable land of Egypt, and that there is now a larger tract of cultivable soil E. and W. from the river than at any previous period. This I shall endeavour to illustrate by a similar section, in which it will be seen that if the Nile, rising from its ancient bed, AB, No. 3, inundated the country in the direction and at the elevation EF, it would, when raised to CD, its modern bed (the land being also raised in proportion to G), extend its inundation on the line GH to a far greater distance over the *Hajer* or slope of the desert, and give an additional tract of cultivable land from F to H. That this has actually taken place I have satisfactorily ascertained by excavations, and by observing the quantity of alluvial deposit accumulated round the base of ancient monuments, and by a comparison of the height to which the water now rises and formerly rose in the nilometer of Elephantine.

In the plain of Thebes are some colossal statues of Amunoph the Third, of which two still occupy their original site, and one of these has long been known under the name of the "Vocal Memnon." They stood on either side of the *dromos*, leading to a temple built by that Pharaoh, and at intervals, between them and the temple, were other colossi, statues, and tablets, long since thrown down or mutilated, and nearly

covered by the alluvial deposits of the inundation. Their relative positions may be better understood from the plan No. 4, in the plate, where it will be seen that before the temple A are the tablets BC, and 420 feet beyond are the fragments of a colossus, E; then at the distance of 220 feet is another fallen colossus, G, and as a pendent to it a group of comparatively small figures cut out of a single block at F; the colossi H I, which are still standing, being 300 feet further forward, and appearing to terminate the dromos.

The temple is now surrounded by alluvial soil, and the water and mud of the inundation extend to the distance of 600 feet behind it. But when erected, about the year 1420 B. C., not only the body of the temple, but the dromos, or paved road leading to it, as well as the base of the colossi H I, were above the reach of the inundation, and the statues at F, which are still erect in their original position, were exposed to view, though now buried to their waist in the alluvial deposit.

Indeed, I believe this dromos to have been a continuation of the "royal street," mentioned in some papyri found at Thebes, which, crossing the western portion of the city, communicated by means of a ferry with the temple of Luxor, founded by the same Amunoph, on the other side of the river; as the great dromos of Sphinxes, connecting the temples of Luxor and Karnak, formed the main street in the eastern district of Thebes.

The colossi H I are 47 * feet high, with the pedestal 60, but the alluvial deposit has accumulated, around them to the height of from 6 feet 10 inches to 7 feet, so that they now stand only 53 feet above the plain.† This was ascertained by excavating to the base of the pedestal; and having penetrated beneath it, I found that it stood, not on alluvial ground, but on the soil of the desert, which was paved with sandstone blocks, serving as substructions for the colossus and the dromos. The lower side of the pedestal had not been cut smooth, but was left of a round irregular shape, extending 3 feet 10 inches below the level of the paved

* By sextant, I make the western colossus 47 feet, and the other, by actual measurement, 47 feet 9 inches.

† The ground has sunk at the base, and the statue inclines a little to one side, so that it is difficult to ascertain the exact height of the pedestal.

dromos; but that was of little importance: the main point was to ascertain whether the slope of the dromos corresponded with that of the desert, and this I proceeded to examine. I therefore dug to the base of what I supposed to be part of a similar colossus at F, 300 feet behind the colossus H, which, however, proved to be a group of statues, a circumstance particularly fortunate for my purpose, as they were found to be standing erect in their original position. Their total height was 8 feet 1 inch from the base to the top of the shoulder, the part above that being broken off; they projected 2 feet 10 inches above the level of the alluvial deposit, so that it had accumulated in this part only 5 feet 3 inches. This satisfactorily settled the question I had in view, and gave, in a distance of 300 feet, a difference of 1.7 to 1.9, being an average of 20 inches in 300 feet, or a decreasing ratio of 1 inch in 15 feet, for the talus of the sloping desert plain on which they were placed.

According to this ratio, the basement of the temple itself should stand very little below the level of the alluvial deposit, which indeed agrees with fact, though, as may be supposed, the slope of the desert is not quite so uniform as to accord with the mathematical calculation of an uninterrupted line. It suffices for our purpose to have ascertained that this gradual slope does exist, and that the colossi and the temple standing upon it are buried in alluvial deposit in an inverse ratio as they approach the edge of the desert, and the only inference necessarily is, that the alluvial soil now reaches farther inland towards the desert than it did when these monuments were erected. We do not know how far the outermost colossi were at that time beyond the line of the alluvial deposit; the only conclusion is, that they were above its level, and that the dromos or paved street was also above the highest water-mark, though it is not possible to fix any exact point from which to calculate the annual increase of the perpendicular stratum of sand. Of this we may at least be certain, that all the deposit now existing between the colossi H I and the edge of the desert behind the temple, a total distance of 1900 feet, has been brought there since the reign of the third Amunoph, or within a period of 3260 years.

What has been said, I trust, fully demonstrates these pro-

positions, that the perpendicular rise of the bed of the Nile extends the inundation and alluvial deposit much farther in a horizontal direction, E. and W., at the present day, than at any previous period; that it has always continued to do so; and that, therefore, there is a wider extent of irrigated land now than in former times. I do not, however, pretend that the same quantity of land is cultivated as formerly; this must always depend on the population, the energies of the people, the system followed by the government, and other accidental circumstances; but it is not the fault of the river, nor from any deficiency in the benefits it used to bestow on the soil of Egypt, that much land is left fallow and overgrown with noxious weeds, and the modern inhabitants might profit by the same means of cultivating the edge of the desert by artificial irrigation as their predecessors, if Egypt only possessed the advantages of population, a favourable system of agriculture, and a wise government.

I have made the same observations respecting the extent of the land in other parts of Egypt, all confirming what I have stated, as might reasonably be expected, since the same causes necessarily produce the same effects; and I now proceed to shew the origin of those erroneous notions which proclaim that the drifting sands have curtailed the limits of the arable land of Egypt, and that the desert, constantly encroaching on the soil, threatens to overwhelm the valley of the Nile, and already counteracts the beneficial effects of the inundation. In some parts of Egypt, as at Bahnasá, in latitude 38° 33′, at Kerdasi, a little to the N. of the Pyramids, at Werdan, still further N., and a few other places, the sand of the Libyan desert has been drifted into the valley, and has encumbered the land with hillocks, spreading itself over the fields near the edge of the desert, and sometimes burying trees and buildings to the depth of several feet. This has been particularly the case about Bahnasá; and Denon, who visited it, and witnessed the effect of the sand in that quarter, spread the alarm of its invasion, which has been magnified into the annihilation of the arable land of Egypt. But this evil is only partial; and, as Monsieur Regnier observes, in his Memoir upon the Agriculture of Egypt, published in the great French

work,* "though many have spoken of the encroachments of the sand upon the cultivable soil, it appears to me much less considerable than is supposed; for otherwise many places indicated by ancient writers to have been on the borders of the desert would now be distant from the irrigated land, and the canal of Joseph, after so many ages of bad government, would have been long since filled up." In some places, he adds, this has happened, as at Werdan, in the province of Gizeh, where the sand has advanced to the distance of a league; but the position of the place, at the outlet of a gorge in the Libyan mountains, is perhaps partly the cause of this, an opinion which perfectly coincides with my own observation. In many places, where valleys open upon the plain, the sand is found to accumulate, and sometimes to form drifts upon the land, which, when no precautions are taken, by planting the bushy tamarisk, increase so far as to prevent the overflow of the Nile from covering a portion of the previously irrigated soil; but these incursions of sand are only partial and in particular spots, bearing a very small proportion to the whole valley of Egypt; and it must be remembered that the desert, or gradual slope of the Hajer, between the limestone range and the arable land, is not a plain of moving sand, as some have imagined, but is composed of clay and stony ground, mixed with a proportion of sand, or an old detritus of the neighbouring rocks. On the eastern side of the valley very few sand-drifts are met with, except those seen from Cairo, beyond Heliopolis, and the Birket el Hájj on the Suez road, but these do not encroach upon the arable land, from which they are far distant; and since I have shewn that on the western or Libyan side also the places where sand encumbers the land are partial, it may be readily imagined how slight an effect they must have, compared with the whole extent of the country.

In the Delta, the only sandy places of consequence are here and there on the Libyan shore, and on the coast of the Mediterranean, bearing an imperceptible proportion to the whole superficies of that province; and, indeed, the sand on the coast

* Memoires sur l'Egypte, vol. iv. p. 5.

is not worthy of notice, nor can it be attributed in any way to the advance of the desert upon the land of Egypt. In many countries, as in France, about Dunkirk, the Landes, and other places, as in Scotland, near Nairn, and in several parts of Europe, sand-drifts occur of great size and extent; but the same theories are not formed upon their aggressions; and we have in this a proof how far opinions are influenced by the name and by the idea of a desert. I am far from affirming that no encroachment of the sand takes place; my arguments are only intended to shew that, taking into consideration the relative advance of the sand, and of the alluvial deposit, the balance is greatly in favour of the latter, and the result is, that, whatever partial injury the sand may have in its power to inflict upon certain spots, the extent of the land is constantly increasing, and the number of square miles of arable soil is much greater now than at any previous period.

I must also make some remarks regarding the nature of the desert, which will be found to differ much from received opinion, as the simple mention of ranges of primitive mountains, reaching an elevation of several thousand feet, would suffice to show. I allude now to the desert lying between the Nile and Red Sea; but in order to give a just notion of the tract, and the nature of the mountains in several parts, I must refer to my map, and to the accompanying sections in different latitudes.

The leading characteristic of this desert, particularly in the northern part, is its gradual ascent from the north of the Nile to a certain distance eastward, where you arrive at a plain, nearly level and of some extent, from which all the valleys or torrents running in a westerly direction, empty themselves into the Nile, and those to the eastward into the Red Sea, following a descent in the opposite direction to the coast. This section, taken east and west, about Lat. 29° (see plate No. 7), will explain the appearance of the desert in that part.

The mountains there are all limestone: the ascent from the Nile to A is about 30 miles; the high plain A B is about 16 miles broad, and the descent then commences towards the Red Sea, which is about 50 miles distant.

In that part where the primitive range commences and joins

the secondary hills, in Lat. 28° 26′, the section E and W presents the appearance of No. 8. of Plate.

In Lat. 28° 10′, passing by the lofty Ghárib, which is the highest peak in this desert, having an elevation of about 6000 feet, the section is of the nature as delineated in the plate, No. 9. of Plate.

In Lat 28° from Jebel Ez-Zeit, in the Red Sea, to Jebel Abú Fáyidah, the section has the character of No. 10. of Plate.

In Lat. 27°, crossing the great range of the Ummumfíyah, which is about 5000 feet high, the section has the appearance of No. 11. of Plate.

From a comparison of which it appears, that this desert has one general character in its levels from the river to the Red Sea.

A little above Esneh, about Lat. 25° 10′, the sandstones approach the Nile on the east bank, and, a little farther up, they cross the river near Edfú, whence they continue on either side of the Nile; and at Silsilis are the quarries from which the sandstone used in the temples of Egypt was taken: 14 miles above Ombos, on the eastern bank, the granites appear, and at Eswan, 14 miles farther, they cross the river. Amidst these are the cataracts, a succession of rapids, of which no single fall is more than about 4 or 5 feet.

In Nubia, the valley is very narrow, the rocks of the eastern and western mountains often coming close to the river, and leaving little or no space for the deposit of alluvium; in other places on the Libyan side, the sand covers the whole level space between the hills and the bank, and the character of the country between the first and second cataract is totally different from Egypt. The river, about Kalabsheh, rises between 30 and 40 feet perpendicularly during the inundation, and, after it has subsided in February, the stream runs at the rate of 2 to 3 nautical miles an hour. But I return to the deserts of Egypt.

In going to the western or Libyan desert in the direction of the Oasis Parva, one road passes by the Fayyúm. That province is considerably lower than the valley of the Nile, and the lake Mœris is about 100 or 120 feet below the level of the bank at Benisùef. A section across that part of the country, from the Nile to the mountain-range, lying behind the lake Mœris, has the form represented in No. 12 of Plate.

The hills A A, on No. 13 of Plate, are a continuation of the range B, as may be seen in a map of this district, from which it will also appear, that on leaving the Fayyúm in a southerly direction, or in going from the Nile westward, you gradually ascend, till you arrive at the summit of an elevated plain, which continues on a level, or with slight undulations, for a considerable distance, and forms the extensive table-land of this part of Africa. The Oasis and other valleys, are depressions in this lofty plain; and, when you descend to them, you find the level space or plain of the Oasis similar to a portion of the Valley of Egypt, surrounded by steep cliffs of limestone at some distance from the cultivated land, which vary in height in the different Oases. Those of the southern Oases are much higher, and consequently their level is much lower than of the Oasis Parva, as may be seen from the section No. 13. of Plate, taken nearly north and south.

From this it appears, that the water of the Oasis Parva does not come directly from the Nile, and that we must look for the origin of its springs from a more southerly point. The mountains of the high plain are limestone, the low plain of the Oases is sandstone on clay, and from this last the water rises, and by this it is retained. The limestone mountains of the Thebaïd rest, in like manner, on clay; and thus we may readily imagine, that the water is conveyed from some point to the south, of greater elevation than the Oases, that its escape to the surface takes place wherever the limestone superstratum is removed, and that a continuation of the same bed of clay conducts it northward to the Oasis Parva, occasional opportunities being afforded it for rising, as at Faráfireh and other places on the way.

Though I have represented the mountains as if the table-land of their summit were perfectly level, in order to shew the comparative depressions of the Oases, it is not to be supposed that they are horizontal; if it were so, those of Lower Egypt would be more elevated than in the Thebaïd, which is far from being the case; the mountains of Thebes being 1200 feet above the Nile, which is a much greater elevation than any in the latitude of Cairo or the Pyramids.

From what has now been said, it will be evident, that the

Oases are not fertile spots in the midst of a sandy plain, but depressions in the lofty table-land of Africa, where, by the removal of the superincumbent limestone strata, the water has the power of rising to the surface: nor is the desert a dreary plain of sand, as some have imagined, which has overwhelmed a once fertile country, whose only traces are the isolated gardens of the Oases.

On the Geographical Distribution of Insects. (Concluded from page 183.)

Of Entomological Regions.—In the same manner as when viewed in a physical or political aspect, the surface of the globe is susceptible of being divided, zoologically, into many regions, characterised by the animals found there: but very different methods may be taken to effect this division.

The method adopted by Fabricius, who was the first that made the attempt in relation to insects, is too well known to require repetition in this place; and the same thing may be said of the improved plan of Latreille. These two systems are founded on data inherent in the globe itself; but others may be established solely dependent on insects themselves. There is nothing to prevent us characterising a region according to the families, genera, or species which inhabit it, and even according to a single remarkable species, found there exclusively. This method, however, is likewise attended with inconvenience. In truth, one or more families may predominate in two countries very remote from each other, in which case it would be very difficult to distinguish these countries; or there may be families so uniformly spread over the globe, that they cannot afford the means of distinguishing any one country in particular. Finally, if we employ only one species to the exclusion of others, we will convey so vague a notion of the region so characterized, that it will be impossible to form a real idea of its entomology. These different modes of proceeding may each have its merit in some particular cases, but they cannot serve as a general groundwork for geographical entomology.

It appears the most expedient course to employ none of these two methods exclusively, but to combine them, keeping the first in subordination to the second: in other words, the insects ought to characterize the region, and not the region the insects. No doubt we shall thus obtain very unequal territorial divisions, since we shall often be obliged to form a distinct region of an island, for example, of very limited extent, or to unite immense tracts of country; but by this method, we shall at least approach as nearly as possible to the distribution of nature, which has not placed species in regular compartments. The regions which we shall in this way form, will be the more natural, since they will possess in themselves a greater number of species, and particularly of genera. In those which follow, this proportion is often found to amount to one-half, and sometimes nearly three-fourths.

It is not to be concealed that, what method soever may be adopted, much that is arbitrary must always exist in the establishment of these regions, and that they can only be regarded as such in a relative light. We may, therefore, either strictly confine ourselves to the eight which have been made use of on a former occasion; or, if it were desirable, even such a country as France might be divided into several. It is best to fix on a medium between these two extremes, without attempting t attain an exactness, which, in such a case, is impossible.

Acting on this view, we find, that there are not fewer than forty regions, one of which is common to both continents, 22 peculiar to the Old, and 17 to the New.

1. *Region common to the Two Continents.*

1st, *Arctic Region.*—This comprehends all the parts of the two continents which lie within the northern polar circle. Its entomology is yet but little known, except in relation to that portion of Lapland included in it. The investigations of M. Zetterstedt have shewn, that the latter is richer than might have been supposed. It is probable that this is likewise the case with the northern parts of Asia, although we are scarcely acquainted with one species from that quarter. Iceland would likewise appear not to be so poor as was for a long time supposed. In Greenland, according to Mr Kirby, all the orders have

their representatives, except the Orthoptera and the Hemiptera. In Melville Island (75° North Latitude), besides these two orders, the Coleoptera and Neuroptera are wanting. These statements are perhaps premature, at least in relation to the Hemiptera, for the insect which has hitherto been found nearest the pole is an *Aphis*, met with on the ice about 82° 26′ 44″, nearly 33 leagues from the nearest land, during Captain Parry's Polar Expedition. It had no doubt been carried by the wind from some point of the coast of Greenland, situated to the south of that high latitude.

Among the Coleoptera, the Carabidæ are dominant in this region, as might be expected; in the order Lepidoptera, the genera Colias, Argynnis, and Satyrus appear the richest in species. The Orthoptera are reduced to a few Gryllus, Acridium, and Blatta; the Hemiptera, which are more numerous, are composed, for the greater part, of Cimicides, to which may be added a small number of Cicadidæ, Hydrocoryses, and Aphides. Among the Hymenoptera the Ichneumonides constitute the majority, as they do everywhere, and, after them, the solitary Apidæ. Finally, the Culicides and the Syrphides appear to be most numerous among the Diptera. The greater part of the species are common to the two continents, and some of them are found in the high mountains of Europe, such as Colias Palæno, which is common in Lapland and Iceland, and which Captain Scoresby has found on the coast of Greenland along with Argynnis Dia. The others have almost all the same *facies*, as those of our climates. Finally, this region has a very small number of species which are peculiar to it. It is, therefore, properly characterized, less by the specialty of its entomological productions, than by the smallness of their number.

2. *Regions peculiar to the Old Continent.*

2dly, *The European Region*, bounded on the west by the Atlantic Ocean; on the east by the Oural Mountains; on the north, by the Arctic Circle; and on the south by a line nearly parallel with the 46° N. Lat. Notwithstanding its extent, this vast territory has an entomology of a singularly uniform character. The same families predominate throughout, and the

genera vary but little; even the species are often identical at the two opposite extremities. We find, for example, with very few exceptions, the same Lepidoptera between the Volga and the Oural, as in the vicinity of Paris.* The richest portions of this region are Germany and Hungary; it is these at least which possess the greatest number of peculiar species. The genera proper to this region are pretty numerous, as has been seen by one of the preceding tables.

3dly, *Siberia*, comprised within the Oural Mountains on the west; the great Northern Ocean on the east; the Polar Circle on the north; and the Altai range on the south, which may be considered as forming a part of it. In this country, which is very little known in its entomological relations, except in the neighbourhood of Barnaoul on the Obi, a part of the Daouria of Kamstchatka, and of the Altai Mountains explored by Russian naturalists, the insects shew, in regard to genera, the closest resemblance to those of the preceding region; but the species are, for the most part, entirely different. Here have been collected, in particular, a great number of beautiful Carabi.

4th, *The Mediterranean Region*.—This includes the French coast of the Mediterranean as far as 45°, Spain, Italy as far as the Alps, Turkey in Europe, Greece, the coasts of Asia Minor, Syria, and the shores of Africa, as far as about 30°. Its entomology is one of the best characterized, and consists more especially of Heteromera, almost unknown in the preceding regions, such as Pimelia, Akis, Zophosis, Elenophorus, Scaurus, Mylabris, &c.; a great number of hairy and cylindrical Buprestes, of the genus Jalodes, and in the other families of Coleoptera, Cebrio, Zygia, Onitis, Ateuchus, Amphicoma, Scarites, Siagona, Ditomus, &c. The Cicadæ and Mantes, very rare in the European region, are common in this. It possesses likewise a great number of fossorial Hymenoptera, and its peculiar Lepidoptera belong particularly to Anthocharis, Satyrus (genus Argus), and the genus Dorytis, pecu-

* See Eversmann's Enumeratio Lepidopterorum fluvium Volgam inter et montes Uralenses habitantium. Bulletin de la Société Imperiale des Naturalistes de Moscou. Tom. iii. 1831.

liar to the coasts of Asia Minor and the Archipelago. Papilio Alexanor is found only here, as well as a new species of the same genus (P. Hospiton, Gené) discovered lately in Sardinia.

5th, *Region of the Caucasus and Caspian Sea*, which comprehends Southern Russia, the Crimea, Georgia, Circassia, Armenia, a part of the Steppes of Kirguises, and the country situated between the sea in question and that of Aral. Its climate, soil, and vegetation, the latter rich in alkaline plants, give it a close relation to the preceding region. The same genera appear, mingled with a few new ones. The Caucasian portion, properly so called, appears particularly prolific in beautiful species of Carabus, many of which have been lately discovered by Russian naturalists.*

6th, *The Arabian Peninsula*, respecting which we possess only very imperfect entomological notices. Its Coleoptera and Lepidoptera, as far as we can judge from the information regarding them, communicated by Forskael in the last century,† and more recently by MM. Hemprich and Ehrenberg,‡ differ only specifically from those of the Mediterranean region, and the greater part reappear in Egypt, Nubia, Abyssinia, and even in Dongola.

7th, *Persia, Caboul, and Afganistan.*—It is nearly impossible to assign limits to this region, of which we know scarcely any thing entomologically speaking. Persia, which was visited by Olivier towards the end of last century, appears to possess, in a great measure, the same genera as the Mediterranean region, with peculiar species.

Between this region on the west, the frontiers of China on the east, the Altai Mountains on the north, and the Himalaya on the south, lies an immense territory, Tartary and Mongolia, which no entomologist has yet visited. If we consider the Himalaya as forming part of it, we know that this chain, the highest on the globe, presents a mixture of Indian and Euro-

* See Menetrius, Catalogue des objects d'histoire naturelle recueillis dans un voyage fait an Caucase, &c., and the Bulletin de la Société des Nat. de Moscou.

† Descriptiones animalium quæ in itinere Orientali observavit, &c. 1775.

‡ Klug, Symbolæ Physicæ, &c. In fol.

pean species, along with others which are altogether peculiar to it. We there meet with, among others, our Parnassius Apollo, Pieris Brassicæ, Papilio Machaon, Limenitis aceris, &c. It is not a little remarkable that hitherto no species of Carabus has been observed there.

8th, *China.*—What we know of its entomology is nearly confined to the species which figure in the boxes of insects which the natives sell to Europeans at Canton, species which are always alike, and consisting chiefly of Sagra, Anomala, some large Cetoniadæ and Cicadæ, with Lepidoptera, for the most part identical with those of the Indian Archipelago, excepting two fine species of Papilio (*P. Paris* and *Bianor*), which appear to be found no where else; and *Saturnia Atlas*, the largest known species of its genus.

9th, *Japan*, known in its entomological relations by the voyages of Thunberg and M. Siebold. Many of the genera, and a few of the species of our climates, appear to occur there along with those peculiar to it.

10th, *Nepaul*, which appears to differ entirely in its entomological productions from Bengal, and produces many beautiful insects.

11th, *Peninsula on this side the Ganges*, with the addition of *Bengal and the Island of Ceylon.*—This region is very rich in species, among which are to be remarked, among the Coleoptera, a great number of beautiful Panageus, Orthogonius, Sagra, Paussus, Buprestides of the genera Sternocera, Chrysochroa, Scarabæi of large size, &c. Here also is found a beautiful Anthia (*A. sexguttata*), the only one known to exist out of Africa. The Lepidoptera bear most resemblance to those of the Indian Archipelago, and consist chiefly of Papilio, Pieris, Limenitis, Nymphalides of the genus Aconthea, &c. The Ghaut mountains, which extend along the coast of Malabar, all the portion of Bengal situate at the foot of the Himalaya, and the Island of Ceylon, appear to be the richest parts of this region.

12th, *Peninsula beyond the Ganges.* A region much less known than the preceding, and remarkable for the fine species of Cicindela and Lepidoptera which are occasionally received from it.

12th, *Indian Archipelago*, comprehending under that name the Isles of Sunda, the Philippine Islands, the Moluccas, and New Guinea. This region might admit of being divided, but, entomologically speaking, it is not easy to perceive how to trace the limits of this division. Java is the best known portion of it, and it possesses a multitude of magnificent Coleoptera, among which the gigantic Lucanidæ and Scarabæi are the most remarkable. The genus Mormolyce, the most extraordinary member of the whole order in question, has not been found in any other place than here. This island likewise abounds in Lepidoptera, particularly in Pieris, Papilio, Euplaea, and Nymphalides. The greater part of its species reappear in Sumatra, of which, however, we are acquainted with only a small part; they are again met with at Celebes, but modified both in colours and size, the latter being augmented. The Moluccas, especially Amboina, Ceram, Gilolo, and the neighbouring islands, are the special native country of the Ornithoptera, in particular of O. Priamus. The Philippine Isles have been but little explored, except in the vicinity of Manilla; they appear to have their own insect productions. New Guinea is extremely rich in Papilio. Perhaps the most characteristic genera of this region are the Cicindelidæ of the genera Therates, Collyuris, and Trycondyla, which have not been found elsewhere. While the other orders maintain their fair proportions, the Lepidoptera may be said to have the pre-eminence for variety and the beauty of the species. The two genera, and the families mentioned above, are almost inexhaustible in species.

14th, *Micronesia and Polynesia*, containing the various Archipelagos scattered throughout the Pacific Ocean to the east of the Indian Archipelago. We may place in the same region all these islands, notwithstanding the immense space over which they are distributed. The insects in this quarter are by no means in relation to the vegetation, which is considerably varied and vigorous. None of the orders, except that of Lepidoptera, can reckon more than a small number of species, the greater part of which are no way remarkable. The smallest of these isles, being of volcanic or madreporic formation, are almost entirely deprived of insects. The Lepidoptera recall the

forms of New Guinea or of Europe, with the absence of entire families, such as that of the Danaïdes, likewise of the genera Papilio, Pieris, &c. The Nymphalides predominate over all the other families, and comprise a pretty considerable number of particular species.

15th, *New Holland, Van Diemen's Land, New Caledonia, and New Zealand.*—This region has a very distinct entomology. Notwithstanding its proximity to New Guinea, New Holland differs from it greatly in its insects. In the latter we find neither Danais, Idea, Leminitis, nor a multitude of genera of the tribe Nymphalides. Magnificent species of Pieris, some Papilio and Satyrus, compose the principal part of the Diurnal Lepidoptera, and the nocturnal kinds include many fine species, particularly among the Bombycites. In Coleoptera, numerous genera are exclusively found here; such as Pamborus, Anoplognathus, Diphucera, Ryssonotus, Lamprima, Macrotops, Adelium, and Paropsis. Lamprima, although very abundant in the Island of Norfolk, does not appear to exist in New Zealand nor in New Caledonia; this latter, likewise, possesses Therates. These two islands, in other respects, are infinitely less rich in species than New Holland.

16th, *Upper-Egypt, Nubia, Abyssinia*, and the neighbouring countries on the west; a region but little known, like the greater part of the other African countries. Egypt does not appear rich in insects, particularly in Lepidoptera. Melasomas, among which are to be noticed the genus Eurychora, Pimelia coronata, Graphipterus, and a great number of Gymnopleurus of great brilliancy, are worthy of being signalized among the Coleoptera. Nubia and Abyssinia are much richer, a circumstance to be ascribed to their soil, which is more wooded and better watered. We there find, in particular, a multitude of Anthocharis and other Pierides. The species in general, as has already been intimated, have the closest relation to those of the Arabian Peninsula, and some of the Lepidoptera extend as far as Senegal.

17th, *Region south of the Atlas*, extending to the great desert, and including Belidulgerid, the empire of Morocco, the islands of Madeira, and the Canaries. We scarcely know any thing of the continental part of this region. Madeira and the

Canaries, on the contrary, have been very fully explored, particularly since the voyage of MM. Web and Berthelot, and they are found to have an entomology absolutely analogous to that of the Mediterranean region, and often identical in regard to species.

18th, *Senegambia*, bounded on the north by the river Senegal, on the south by a line drawn from Bissagos, on the west by the ocean, and without determinate limits on the east. The principal parts whose entomology has been explored, lie along the river Senegal as far as the country of Galam, and the sea coast. It is a very rich region, having perhaps more relation in its insect productions to Abyssinia and Nubia than to the following region. It possesses, at the same time, a very great number of peculiar species belonging to all the orders.

19th, *Coast of Guinea*, from Bissagos as far as the river Gabon, a region whose climate is so fatal to Europeans as almost to cut it off from the researches of naturalists, but which is one of the most remarkable on the globe in relation to its entomology. All its productions of this class have a *facies* peculiar to them, although they are allied to the species of Senegambia and the Cape of Good Hope. It possesses numerous Lepidoptera of the highest beauty, particularly Papilio, Danais, Aerea, and the Nymphalides. It is the native county of Charaxes; and among the Coleoptera, Teflus, Goliathus, &c.

20th, *Congo*, a country little known in the light we are now considering, but it appears not to be so rich as the preceding.

21st, *Cape of Good Hope*, as far as the tropic of Capricorn, and consequently including a great part of Cafraria. This region is better known than any other in Africa, and it is very rich, but only in Coleoptera. It is the native country of Mantieora, Platichyles, Dromica, of large species of Moluris and Brachycerus, of a great number of Jalodis and Lepitrix, Anysonyx, Monochelus, which replaces Amphicoma of the Mediterranean region, &c. Its Lepidoptera, of moderate number, consist of some beautiful kinds of Papilio, Danais, Nymphalides, and Satyrus. The genus Pneumora, one of the most remarkable of the Orthoptera, has only been found here.

The insects of all the central parts of Africa are wholly

unknown, and it is consequently useless to establish other regions in this continent.

22d, *Madagascar and the Anjouan Isles.* The entomology of only the south and east of Madagascar is yet known. It is probable that this extensive island has, in this respect, a great analogy to the coast of Africa, opposite to which it is placed; but as we know nothing of the latter, this relationsbip cannot be proved. However this may be, this region is very rich, and possesses many peculiar genera, such as Psilocera, which here replaces Collyuris of the Indian Archipelago, Thyreopterus, Hexodon, and Bolybothris, species of Buprestides with very broad elytra, and of a *facies* unknown elsewhere. A multitude of fine Cetonias, Curculionidæ, and Lamias of large size, have likewise been discovered here. The Lepidoptera mostly consist of Acrea, Papilio, Vanessa, and Nymphalides; some of these are the same as those of Senegal and southern Africa. In the islands of Anjouan the genus Idea re-appears, although it belongs more particularly to the Indian Archipelago.

23d, *Isles of Mauritius and Bourbon*, to which may be joined the Seychelles Islands. Notwithstanding their limited extent, there is a necessity for making a separate region of these islands. Their entomology, which is neither rich nor varied, has an ambiguous *facies*, at once African and Asiatic. Their Coleoptera, at least in conspicuous species, are reduced to a restricted number of Curculionidæ and Longicornes. In Lepidoptera we do not find a single example of Pieris, Colias, or Acrea; and in regard to Mauritius and Bourbon, only one species of Papilio occurs in each. The Seychelles islands are not very remarkable for any thing, except a fine species of Phasma, which is confined to them.

3.—*Regions peculiar to the New Continent.*

24th, *North America, English and Russian*, bounded on the north by the polar circle, on the south by the great lakes of Canada, and by a line prolonged westward to the Aleutian Islands inclusively. This region presents a near analogy to the corresponding one on the Old Continent. It affords the same genera, and often the same species. In Canada, how-

ever, we meet with a few remarkable genera, and which are proper to it, such as Scaphinotus and Sphaeroderus, which represent our Cychrus. The Aleutian Isles, with a few of whose entomological productions Eschscholtz has made us acquainted, have scarcely furnished more than a few Carabidæ without a single new form. The large island of Terra-nova should be included in this region.

25th, The *United States*, to the Mississippi and the Missouri on the west, and the Gulf of Mexico on the south; a region not so rich as its geographical situation might lead us to suppose. The Coleoptera are dominant here, and present a mixture of European and intertropical forms. Here also we find Phanæus by the side of Geotrupes, Rutela by the side of Melolontha, and Gymnetis along with Cetonia, &c. The most conspicuous of the genera of this order proper to the region in question are Pasimachus and Dicælus. The Lepidoptera, particularly the diurnal section, are singularly limited in numbers; scarcely eighty species are known, and many of these reappear in Brazil, and a few in Europe; but this region possesses a peculiar and very remarkable group of Papilio (P. Troilus, Asterias, Glaucus, Calchas, Philenor, Villersii), the analogues of which exist nowhere but in Mexico and the Antilles. Among the nocturnal kinds we remark beautiful species of Saturnia and Agarista, a genus of which the proper native country is New Holland. The other orders, as far as can be judged from the works of Say, are likewise but moderately supplied.

26th, *Region of the Rocky Mountains*, bounded on the east by the Missouri and Mississippi, on the west by the ocean, north by New Hanover, south by Old California; a region very imperfectly known; in its northern quarters bearing considerable resemblance to Europe, and appearing to be very rich in Melasomas in its southern parts. The most remarkable of the genera peculiar to it is that of Omus, belonging to the tribe of Cicindelidæ, discovered by Eschscholtz in New California.

27th, *Old California, Mexico, and Central America*, as far as the isthmus of Panama. This region admits of being subdivided at least into two others. California, although arid,

appears to have a special entomological Fauna, in which the Melasomas are dominant. Mexico properly so called, presents every kind of climate and vegetation, from the warm lands (*tierras calientes*), which form a zone of several leagues in breadth, to the cold lands (*tierras frias*), where almost a perpetual snow prevails. The shores of the Gulf of Mexico are almost the only part that has been explored, and it is extremely rich, presenting, besides many peculiar genera, a multitude of species belonging to the genera of Brazil and Guiana. Central America is scarcely known.

28th, *The Antilles.* This Archipelago, forming an arc between North and South America, is connected with both by its entomology. We again meet with many species of the mainland, and of the United States; but the large islands have also their appropriate insects. Papilio Lycoreus, and P. Imerius, for example, have hitherto been found only at Hayti; Nymphalis Rogeri at Cuba; Scarabæus Hercules exists in the greater part of the Archipelago, &c. The smaller isles, on the contrary, appear very poor. The most interesting discovery made of late years in this Archipelago is that of a true species of Carabus, which has lately been found at Porto Rico.

29th, *Valley of the Rio Magdalena, and that of the Lake Maracaybo*, including the Andes which separate them. This region, which has been but little examined, is particularly rich in Coleoptera; although its species belong in general to the same genera as those of Guiana, they are all different. In the number appears a Scarabæus as large as S. Hercules, and nearly related to it, but altogether distinct. No other place has hitherto afforded the fine genus Psalidognathus. Its Lepidoptera have some affinity with those of the Antilles, but many of them are exclusively appropriated to this region.

30th, *Province of Caraccas*, from the sea of the Antilles to the Orinoco on the south. We are acquainted only with a few Lepidoptera, which are very distinct from those of Guiana. The Llanos, which extend towards the south, are unknown in their entomological relations, but they must be very unproductive.

31st, *Guiana*, comprehending under that name all the territory comprised between the Orinoco, the Rio Negro, the

River Amazons, and the sea. This region has been explored only along the sea-coast: it is very rich in Lepidoptera and Hymenoptera, but inferior to Brazil in Coleoptera. Its species have a close analogy to those of the latter country, but nearly three-fourths are peculiar to it.

32d, *Eastern Brazil*, bounded on the west by the Rio Tocantin and the Parana, and on the south by the province of southern Rio-Grande, which is not included in it. This region may be divided into as many sub-regions as there are basins formed by the principal rivers. The portion lying between the ocean and the Sierra de Mantiquiera, is the richest country in the world for every order of insects, but particularly for that of Coleoptera.

33d, *Western Brazil*, bounded by the Amazon on the north, Peru and Bolivia on the west, Paraguay on the south, and the preceding region on the east. This immense territory is almost unknown as to its entomological productions.

34th, *Peru*, with its present political boundaries. The whole portion of this region, lying between the Andes and Pacific Ocean, is extremely arid, with the exception of a few valleys, and appears to be extremely rich in Melasomas. The Andes themselves, and the countries to the east, are unknown.

35th, *Upper Peru*, or *Bolivia*. M. Lacordaire has seen none of the productions of this region, except the pretty rich collections made by M. d'Orbigny in the province of Santa Cruz de la Sierra. The Coleoptera and Lepidoptera have a *facies* entirely Brazilian. Many of the species are even identical, some with those of the neighbourhood of Rio Janeiro, others with those of Cayenne. The species proper to the country consist chiefly of Melasomas.

36th, *Paraguay*, *Southern Brazil*, and *Eastern Banda*. All of these have a very intimate relation to the insects of Brazil, such as occur in this region differing in general specifically, particularly as we approach the La Plata. In the province of Rio Grande, numerous species begin to appear, unknown in the vicinity of Rio Janeiro, although belonging to the same genera.

37th, *Tucuman*, bounded on the north by Upper Peru, on

the south by a line parallel with 35° S. Lat.; on the east by the preceding region, and Buenos-Ayres; on the west by the Andes. This region is the true country of the Melasomes in America, and especially of Nyctelia and the allied genera. This is owing to its arid, sandy, saline soil, which presents characters of a more decided description in proportion as we approach the Andes. After the family in question, the Carabidæ are most numerous. The Lepidoptera are also very numerous, but they offer nothing remarkable.

38th, *Buenos-Ayres*, from the banks of the La Plata to those of Rio-Negro in Patagonia, by the 42° S. Lat., and about 200 leagues west of the Atlantic. We distinguish this region from the preceding, on account of the peculiar characters of its soil, which is in general argillaceous and covered with grass as its only produce. The insects also are in very small numbers. During a long residence at Buenos-Ayres M. Lacordaire did not collect, in a radius of 40 leagues round the town, more than about 300 Coleoptera. The Carabidæ are dominant, as in Europe, and the Melasomes are restricted to a few Scotobii. All the Lepidoptera met with, are a few Pieris, Colias, and Vanessa. Intertropical forms, however, are still represented here by two or three beautiful species of Phanaeus, and a small number of other genera.

39th, *Chili*, with its present political limits, including the Andes: althongh bordering on Tucuman, from which it is separated only by the Andes, this region differs much from the latter in its entomology. The Melasomes are here rather numerous, but represented by other species. This is the native country of Chiasognathus, one of the most remarkable of the Coleopterous order. We here find also a few equatorial forms, such as Pyrophorus, Heliconia, Castnia, &c.

4th, *Patagonia*, from Buenos-Ayres, Tucuman, and Chili, to Cape Horn; a region altogether unexplored. Fabricius has described a few Carabidæ from Terra-del-Fuego; but the entomologists of the present day know nothing of them.

By recapitulating these regions, it will be seen, that 14 are situated in the Northern Hemisphere beyond the tropics, 19 between the latter, and 7 in the Southern Hemisphere. We have overlooked a number of small islands scattered over the

ocean, such as Tristan d'Acunha, Ascension, St Helena, &c., because we are completely unacquainted with their entomology. Many of the above regions, particularly such of them as are intertropical, ought hereafter to be subdivided, when we become more intimately acquainted with the habitations of insects.

Of the Diseases of France in their relation to the Seasons, or Sketch of a Medical and Meteorological History of France. By Dr Fuster.*

It is a principle of medical philosophy long since established, that relations of causality and dependence exist between the physical characters of the seasons and the nature of the common maladies which correspond to them.

Endowed with its proper nature, each season determines, in the animal economy, an order of particular movements; it leaves there, on passing, impressions so much the more marked and more durable, as its action is exercised unmixed, more strongly, and for a longer period. The season which succeeds comes in its turn to impress on living bodies a different series of new movements, and by the aid of these oscillations, balanced between the limits which are assigned to them to traverse, there is what may be reasonably called a medical year.

This principle of the agreement of the seasons with what are called the small epidemics, fertile in clinical applications, so proper to guide the physician in the diagnosis of diseases, so useful to direct him in the study of their causes, so powerful to elevate him to the knowledge of their true nature, and to the determination of their better treatment; this principle, I say, had been already laid down by Hippocrates. It is always found holding a high place in his admirable books of Epidemics; and in his *Treatise of the Air, Water, and Localities*, one is still more surprised to read the following passage: *Astronomy contributes not little, but very much, to the medical art; since the stomach changes with the seasons of the year.*

* This is a "Report of a manuscript work presented to the French Academy of Sciences, and published in the Comptes Rendus des Seances de l'Academie des Sciences, 23d September 1839, pp. 393," &c. (Commissioners MM. Arago and Double, the Report being by the latter.)

Galen, but above all his followers, by a strange association of this principle with the philosophy of Pythagoras, exaggerated both the sense and the employment of it. The principle, consequently, was soon neglected, abandoned, forgotten; then resumed, appreciated, praised, or even exalted beyond measure; finally, during the last fifty years, the word has scarcely been heard in our schools, academies, and books.

Other sciences, as well as medicine, will assuredly furnish examples of these vicissitudes. The human mind is thus made; it only avoids one defect by the contrary excess. Luther has said, with all the fire of his genius, after having proved it, perhaps, by the extravagances of his character,—"The human mind is like a drunk man on horseback; when you lift him to one side, he falls to the other."

If the principle of the constitutions of the seasons, and of the constitutions of diseases which correspond to them, has undergone such alternatives, it is because it has not yet been sufficiently elucidated, explained, developed; it is because the theory of this point of medical philosophy, hitherto imperfect, has not been formulized with sufficient clearness, with sufficient precision.

In the work at once of great length and high interest which we have just examined, M. Fuster has had for his object to fill up this lacuna; he has wished to establish the relations which really existed between the characters of each of the seasons, and the nature of the common diseases of the year under the climate of our country. He has thus proposed to trace equally the meteorological and medical history of France. None has hitherto undertaken a similar task.

Physicians and meteorologists cannot study atmospheric phenomena according to the same process, the same principles, or with the same view. The meteorologist endeavours to know the different atmospheric states in themselves, and independently of all the modifications which they cause sublunary beings to undergo. The physician, on the contrary, while noting the peculiar characters of the seasons, wishes to appreciate them in their action on the whole animal economy.

In meteorology, it has been often observed, that we can-

not exhibit a dogmatic treatise which science avows; a work to be recommended on the physical constitution of the seasons, on their characters, their elements, their march, and their influence.

One of the members of the Commission, we know, possesses the materials of a large work on meteorology. Why cannot the friendship which has otherwise so great an empire over him, determine him to the prompt publication of a book so desirable, and which would be so useful?*

A didactic work on the subject is not less wanting to physicians than to astronomers. We have no methodical treatise, no philosophical summary, on the annual morbid states, on the sickly constitutions of the seasons.

It is not because a sufficiently large number of observers of different countries have not been employed in this double kind of studies. Many undoubtedly have undertaken to follow attentively the atmospheric phenomena of each season, and to mark the general diseases which are associated with them. Such, among others, are Sydenham, Van Swieten, Ramazzini, Huxam, De Haen, Stoll, Storck, the physicians of Breslau, Duhamel, Lepecq de la Cloture, the ancient Royal Society of Medicine. But in these we have only the scattered materials for the construction of the edifice to be raised.†

On one side, then, meteorologists of high name have laboured sufficiently long to elucidate this question, and one point on which they are all agreed, is the immense difficulties which this subject presents.

Reaumur first ascertained that all thermometric variations of 4° R. (9° Fahr.) notably affect the economy; so that, according to this philosopher, 4° more or less of his thermometer will produce on the general sensibility of the skin, an effect ana-

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* The work alluded to is the long promised treatise of Arago on Meteorology.—Edit.

† During ten consecutive years, the Reporter of the Commission has published every three months, the history of the reigning diseases at Paris, with the characters of the prevailing seasons; it is due to truth to declare, that if this work has not enabled him to ascend to the causes of the great epidemics, it has been very useful for directing him in his clinical exercises.

logous to that which a tone determines on the special sensibility of hearing.

Cassini, on his part, attempted to shew to what degrees of the thermometer, at Paris, the different sensations of a moderate, a strong, and an excessive heat correspond.

Finally, Jerome de Lalande has proposed a thermometer graduated precisely according to the relations which exist between the habitual movements of the external temperature, and the modifications of the animal sensibility.

Every one knows the numerous and minute meteorological observations collected with no less perseverance than exactitude by astronomers on many points of the globe. These tables, every day more substantial, and which attest at once the persevering zeal and varied knowledge of the superior men who drew them up, cannot be too much multiplied. It is on them that the future success of meteorological science rests.

M. Fuster, on entering upon the first part of his subject, has skilfully availed himself of the labours we have just alluded to. He has joined to them many experiments of his own, and which have for their special object the research of the action of the physical qualities of the air on man, and in a word, the study of the organic action of the seasons.

The physical qualities of the atmosphere, and temperature at their head, undoubtedly play the first and principal part in the pathogenic influence of the seasons. But we ought to distinguish well in this arduous question, absolute from sensible heat. Absolute heat is that whose intensity is measured by the rise of the liquid in the thermometer; sensible heat, on the other hand, is estimated by the impression produced on our organs. Now, the effect of sensible heat is not always proportional to the degree of absolute heat. Considerations borrowed, some from meteorology, others from the organism, explain this difference. Thus, independently of all variation of absolute temperature, atmospheric electricity, the dryness or humidity, the tranquillity or agitation, of the air,—considerations drawn from meteorology; and different anatomical, physiological, and vital modifications, such as cutaneous transpiration, innervation,—considerations borrowed from the organism, cause the sensible heat to vary much.

It is, above all, from the association of the temperature with other determinate conditions, that the prevailing diseases arise. These conditions principally are,—the climate, the sun, the winds, the waters, the aspects, the agricultural productions, light, electricity, magnetism, &c.

Independently of these peculiar characters, under the influence of the conditions enumerated, the characters of the seasons also react on each other, in so far as each of them transmits emanations of its influence to the season which precedes it; in the same manner as each inherits, in its turn, and in different degrees, the action of the season which has past. This modifying influence, marked by certain observers, had not, however, sufficiently attracted attention. M. Fuster has set this truth in its clearest light; he has, so to speak, fixed the theory, established the doctrine of it. He has shewn that, in this point of view, the principal characters of the seasons are mingled, and grow together at the commencement and end of their course, by the reciprocal penetration of their prevailing qualities. This intimate fusion produces a mixed atmospherical constitution, which participates at once of the characters of both seasons. Consequently a middle or mixed atmospherical constitution opens and closes the march of each season.

Besides, we may remark here, as to the seasons in particular, that nothing is abruptly, and with precision, divided in nature; she almost always proceeds in the way of provident transitions.

The two elementary seasons do not contribute equally to this mean constitution. The relations of their relative preponderance acknowledge also certain general laws, the formulas of which M. Fuster gives, but which it would be too tedious to develop here.

What the author advances concerning the seasons, he applies equally to climates or regions. He assigns to them also proper characters, and characters transmitted by the different relations between the neighbouring climates. A capital difference distinguishes the climates and the seasons;—it is, that the climates are fixed, while the seasons are transitory.

M. Fuster has reasoned hitherto on the supposition, that the

seasons were regular. It is, however, certain, especially with us, that this supposition constitutes the rarest cases: our seasons vary very frequently, both in their characters and relations.

To know completely the pathological action of the seasons, it is necessary, then, to consider both the ordinary atmospheric phenomena, and those which are unusual, or the intemperatures and the modifications which proceed from the influence of each locality.

Certain special physiological determinations; certain profound organic modifications; certain distinct general diseases correspond, under certain conditions, to the action of the different seasons. These common maladies of the year are generally divided into two classes; one of these comprehends the diseases of the regular course of the weather: these are the medical constitutions of the seasons: the other includes the diseases of the irregular weather.

These two classes of diseases resemble each other in many points: they acknowledge the same causes; they affect the same masses; they are developed and grow by degrees; they vary much in their forms, but very little or not at all in their nature or essential characters.

During regular seasons, and under temperate climates, enjoying the four seasons, the prevailing morbid states, at the beginning and end, unite or combine with the morbid state of the neighbouring season; so that, at the transitions of each season, one observes a mixed, complex, pathological state, resulting from the concurrence of the action of the season which ends, and that of the season which commences. Therapeutics, we may observe in passing, profit much by the sagacious determination of these delicate variations, in perfect correspondence with the facts very frequently remarked by attentive observers.

Neither the regular seasons nor the irregularities of the weather necessarily engender the affections which are correlative to them; they only produce these diseases when the living body, by a predisposition, oftener observed than determined, but which a well directed study of the temperaments allows to presume, is rendered sufficiently accessible to their impressions. What is certain is, that the wise precepts of the

hygiene have for their object to render the economy refractory to this pathogenic influence of the seasons and of irregularities in the weather. We may add, that the season which arrives gradually modifies, and changes in the way of provident succession, the disagreeable impressions of the season which has passed.

A capital fact evidently results from the numerous observations of M. Fuster on this subject, which is, that the action of the temperature, and the same must be said of the other atmospheric qualities of the seasons, only subjects to its sway the modifications of the economy, in as far as this action is at once strong, continued, and durable.

In a word, the results of these combinations, seized with perspicacity, between the different meteorologico-medical data of France in general, studied in its three great regions, the north, centre, and south, and of Paris in particular, justify the principles of the author on the morbid action of the different atmospheric states; they offer, in the present situation of our knowledge, the most faithful and complete table of the meteorologico-medical constitution of our country.

In the work of M. Fuster, the annual diseases of France are studied in their turn according to the same plan as the seasons. We shall give, as we have attempted hitherto to do, the general results of the numerous facts collected by the author.

In the spring, meteorologically characterized by atmospheric vicissitudes of all sorts, participating of the cold of winter in the beginning, and the heat of summer in the decline, the prevailing diseases are inflammatory catarrhs in the first period, and bilious catarrhs in the second. The organs of respiration and digestion are their principal seat.

During the summer, the development of the heat soon causes bilious affections to prevail. Nevertheless, as the summer of France, usually variable, participates also more or less of the characters of the spring and autumn, the bilious affections are always combined in a remarkable degree with the phlogistic and mucous element. The gastric, hepatic, or intestinal apparatus is more particularly attacked.

In autumn, the increase of the atmospherical variations restores to the first rank the catarrhal affections of the spring. There is, however, this great difference, that in spring, a sea-

son variable and cold, preceded besides by the intense cold of winter, the catarrhal affection is united with the inflammatory affections; while in autumn, a season variable and warm, preceded on the contrary by summer, the catarrhal affection is conjoined with the bilious affection. This last combination is besides very susceptible of degenerating into severe states, and of putting on pernicious forms. The abdominal organs, the intestines among others, more especially suffer.

Finally, during winter, when the cold prevails, the phlogistic affections are most common; and as, with us, the cold is almost constantly attended with fogs, rains, snow, and great atmospherical vicissitudes, the inflammatory affections accompany the catarrhal and mucous: two morbid states analogous, but which are not, however, identical. The sanguineous system, and still more the mucous, of all the organism, are then most exposed.

After having determined the annual diseases of France, taken in mass, M. Fuster compares, in particular, the differences which characterise the annual diseases of the north and south. In the north as in the south, where the four seasons are very distinct, you also find the corresponding diseases well pronounced. The four seasons in the extreme regions are, however, far from always offering an equal preponderance. In the north, the winter is the rudest and longest season; in the south, on the contrary, the season of summer is invested with these characters. The spring and autumn yield to the influence of the two powerful seasons which separate them: thus, while in the north the winter encroaches at once both on the spring and autumn, and extends far into their domain the empire of its qualities, in the south the spring and autumn are invaded by the warm season, and more or less strongly impressed with the burning seal of summer. Hence it results, that in the climates of the north, the meteorological year, more or less analogous to the polar year, takes much more from the winter than from the summer; while in the climates of the south, the meteorological year, participating more or less of the equatorial, is more allied to the summer.

Thus of diseases and their differences. In the north, the inflammatory affections, the necessary attendants of winter,

are the prevailing annual diseases. In the south, on the contrary, the bilious affections, the inseparable companions of summer, constitute almost the totality of the reigning diseases.

M. Fuster examines for each season the nature and relations of the concomitant maladies, and he demonstrates, according to his own observations, and all the observations previously collected, that the medical constitutions of the different regions of France correspond, and are exactly suited, without prejudice of local varieties, to the meteorological constitutions of these same regions.

For diseases and seasons, he establishes the existence of complex states, of mixed constitutions, subject to the same rules, and obeying the same laws. There is no need certainly to say, that these two elementary constitutions do not contribute to the mean medical constitution of the mixed in equal proportions. The relations of their preponderance, the supremacy of the predominant elements, acknowledge in their turn certain general laws whose formulas M. Fuster has traced.

To this exposition, which we have reduced with regret to its narrowest limits, we shall subjoin certain clinical considerations, which appear to us proper to give weight and credit to the doctrine of the constitutions of the seasons, and of the medical constitutions which correspond to them.

Besides the high influence which the characters of the seasons exercise on the nature of the general or common maladies, these same seasons, and the concomitant pathological element, experience shews, are vigorously felt on the accidental diseases, the intercurrent or sporadic maladies, those which here and there present themselves to the physician. It often happens, for example, that a pneumonia accidentally developed in the height of summer, under the reign of a well-marked bilious constitution, puts on by degrees the impression of this bilious constitution. It takes its characters; it presents its symptomatology; it claims its therapeutical indications.

This same influence of the meteorologico-medical constitution very often extends over external diseases, large sores, wounds, and the consequences of the operations which they occasion. Desault and Bichat, who expressly remarked it, frequently turned it to the advantage of their patients. The rich collec-

tion of the Memoirs of the ancient Academy of Surgery, also furnishes many examples of it.

Hippocrates, we have said, had already remarked the fact of the wonderful agreement between the seasons and the common diseases. I add, that he pushed very far the determination of the varied circumstances which bear relation to it; and one may affirm, without hazard, that his genius had measured all its importance, discovered almost all its consequences, and created so to speak its entire doctrine.

He had established a sort of analogy or relation between the action of the diurnal revolution and the action of the annual revolution of the sun relatively to the production of diseases: *As the year exhibits periods of Disease, the day also presents the same.* The morning was, in his opinion, analogous to the spring; noon represented the summer; the evening corresponded to autumn; and the night to winter. Thus the year was only a long day, and the day a contracted year.

I purposely insist on this order of considerations. Many of our classical writers, who belong by their doctrines to the school of Hippocrates, have confirmed and developed by numerous observations this clinical fact:—Such among others, not to cite the most modern, were Sydenham, Piquer, Triller, Baillou, Ramazzini, Huxam. Hence result the following propositions:—diseases appear differently during the day and night, patients are also differently affected during these periods; and farther, physicians have remarked in general, that even at the different epochs of the single diurnal period (that is the time which elapses between the rising and setting of the sun), diseases exhibit different phenomena, the value of which practitioners can well estimate. The inflammatory diseases, those which are characterised by the exaltation of the vital powers, usually undergo their most powerful exacerbations towards morning; they find also, at this epoch, their most habitual invasions.

The catarrhal and mucous fevers, both remarkable for the slowness of their movements, and the atony which accompanies them, begin and are exasperated most frequently on the approach of night; thus the tertian of Hippocrates.

The bilious fevers, which in their characters seem to hold

a mean between the inflammatory and catarrhal diseases, have their paroxysms and most common invasion towards mid-day. Their paroxysms approximate more nearly, whether in the morning or at night, according as the sthenic or asthenic diathesis is more predominant.

The accessions of intermittent fevers commence most frequently during the day. It would be sufficiently difficult to cite many of such invasions during the night.

The slow fever which accompanies large internal suppurations has its exacerbations in the evening, and it is during the night that patients are most tormented by them. The symptomatic sweats shew themselves almost exclusively in the morning, a little before the dawn.

Ramazzini, in his *Récit de la Constitution épidémique* of 1690, describes an ataxique* remittent fever, all the symptoms of which took a very alarming intensity on the approach of sunset. The patients were cruelly tormented all the night; one would have thought they were going to yield their last sigh. In the morning, with the first rays of the sun, all these symptoms ceased, and the patients could rise, go out, and walk about without assistance, in the sun; *like snakes erect, enjoying the solar beams*, are the words of the author.†

Huxam, in his beautiful *Treatise on Angina Maligna*, the angina gangrænosa so frequent in England, has well remarked, that this malady, during its whole course, presents its exacerbations in the evening. He also adds that, when the patients are very well all the day, remarkable aggravations of all the symptoms take place in the evening.

We resist, with regret, the desire of farther multiplying these citations, eager as we are to present to the Academy an approximation which has lately occurred to us between this order of facts and the valuable discovery of M. Daguerre. Doubt is the school of truth; hence it is so useful in the sciences; it is only in the dubitative form that we wish to hazard these approximations.

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* When the prevailing symptoms in fever indicate derangement of the nervous system, pervigilium, restlessness, tremors, or spasms, &c. the name of *Fievre Ataxique* is given to it by recent French authors, corresponding to the nervous or brain fever in this country.—Edit.

† Constit. Epidem., 1690, § 10, p. 126, and § 39, p. 143.

According to M. Daguerre, the hours of the morning and of night, equally removed from mid-day, and corresponding to similar heights of the sun above the horizon, are not, however, equally favourable for the production of photographic images. Thus, in all the seasons of the year, and by atmospherical circumstances in appearance exactly similar, the image is formed a little more quickly at 7 in the morning, for example, than at 5 in the afternoon; at 8 than at 4; at 9 than at 3.

It is not from mere curiosity that we have noticed this approximation. The chemical action of light might well be more foreign to these facts of the invasion and exacerbation of certain diseases at different hours of the diurnal revolution. The human body is more sensible than the most exact instruments, and, above all, its sensibility is of a different description. We see, then, as was previously presumed, that the discovery of M. Daguerre and his new reagents will receive more than one scientific application. This discovery will exercise its influence on many phenomena, on those even which form the source of physiology and medicine.

We would particularly recommend to M. Fuster this new field of meditations; it enters quite naturally into the series of his researches, and no one is better fitted to improve it.

The great fact of the diurnal movements does not present itself only in certain acts of human life; the exciting action of light and heat is often manifested in a different, and sometimes even opposite manner, not only on individuals, but also on species among animals and vegetables. We find nocturnal species among the mammifera, birds, reptiles, mollusca, and insects.

We also meet with the phenomenon of diurnal movements in many functions of the vegetable life; every one knows the sleep of plants, the horologe of Flora, &c. Draparnaud has observed, that at the end of autumn, when the temperature begins to fall, the flowers of the *Ipomœa violacea*, and of the *Mirabilis*, which, till then, are only open during the night, open at this epoch also during the day.

The most exact observations have established these diurnal movements in the different phenomena of meteorology. Such are the diurnal motions of the compass and the barometer; it

is at the warmest hours of the day that hail is formed most abundantly, and in Europe it falls almost constantly during the day.

It is perhaps by dwelling on these analogies, by approximating and comparing them, that one arrives at the knowledge of the laws which these phenomena obey, and at the discovery of the general cause which produces them.

The work of M. Fuster, we have seen, by the important facts and useful experiments which it contains, as well as by the reflections to which it gives rise, is a real service rendered to medical philosophy, and to the science of clinical observation.

If we have succeeded in presenting in a lucid manner a summary of this important work, in shewing in their true light the new facts which it details, and the excellent direction which it cannot fail to impress on our medical studies; if we have been able to convey our own conviction into the mind of our colleagues, each will have already concluded with us, that the work of M. Fuster merits the approbation and encouragement of the Academy.

On the Adulteration of Fixed Oils. By William Davidson, M. D., Glasgow.* Communicated by the Society of Arts of Scotland.

The adulteration of fixed oils is of very frequent occurrence, and injurious to a great extent, in many of our manufactures; but the detection of the fraud can be made only in a few instances with certainty, and, in most cases, an examination of their sensible properties is the only means we possess for forming an opinion. The imperfect nature of our knowledge upon this subject may be accounted for, partly from the intimate similarity of the various species of fixed oils in their chemical characters, and partly from the difficulty of procuring unsophisticated samples. It is, therefore, of importance that any experiments upon this subject should be made known, for,

* Read before the Society of Arts for Scotland, 20th June 1838.

however unimportant they may be in establishing precise rules for the detection of the very numerous falsifications, they may in some degree contribute to their ultimate improvement. Before describing the experiments which have been made, I shall take notice of the most important facts that are recorded upon this subject.

M. Poutet, in 1819, published a memoir upon the falsification of the oil of olives.* He states, that "the acid nitrate of mercury perfectly solidifies the oil of olives, and leaves fluid and colours yellow-red those of all the oleaginous grains." He directs the acid nitrate of mercury to be prepared by dissolving without heat, 6 parts of mercury in 7½ of nitric acid, sp. gr. 1.350, 2 drachms of this solution are to be mixed in a phial with three ounces of olive oil, and the mixture is to be agitated every ten minutes for an hour or two. In several hours, varying with the season, it congeals into a yellowish-white mass, and in twenty-four hours is completely solidified, and is more white. When olive oil is adulterated with the oil of any of the oleaginous grains, it does not take a solid mass, but a portion remains liquid, of a yellowish-red or brownish-red colour; and its quantity indicates the proportion of foreign oil. It has since been shewn by M. Boudet that castor oil, and by M. Lescalier, that oil of almonds, are solidified by the acid nitrate of mercury. The presence of castor oil, however, can be determined by another method, which was discovered at the same period by M. Planche and M. Rose, viz. its solubility in all proportions, in alcohol of sp. gr. 0.817.

Rousseau has also pointed out a method for recognising the purity of oil of olives, which is founded upon the property of oil of olives being a better non-conductor of electricity than other vegetable oils. He has demonstrated that its conducting power is 675 times less than any other vegetable oil; but it must be recollected that the stearine of animal fat is possessed of the same property. M. Felix Boudet published, in 1832, a thesis, shewing that the solidification of oils, by the acid nitrate of mercury, is owing to the hyponitrous acid

* Annales de Chimie et de Physique, Tome xii. et Journal de Pharmacie, Aout 1819.

which exists in it; and he found that one part of this agent is sufficient to transform 200 parts of olive oil into a solid body.* He prepares hyponitrous acid by distilling the nitrate of lead, and, on account of its volatility, he adds three parts of nitric acid sp. gr. 1.350 to give it fixity. He has also found that the following oils are solidified by the hyponitrous acid, viz. the oil of the sweet and bitter almond; the oil extracted from the nut of the Corylus avellana; the oil of the Anacardium occidentale; castor oil, and the oil extracted from the seeds of the Brassica campestris. M. Boudet is of opinion that the siccative oils may be distinguished from the non-siccative by the hyponitrous acid; for, with the exception of castor oil, the siccative oils resist its action, while those which are non-siccative are solidified by it. He calculates the proportion of adulteration by the retardation of the solidification. Thus, in mixing 1 gr. of hyponitrous acid with 100 grs. of oil of olives, he has remarked that the solidification was retarded forty minutes by $\frac{1}{100}$ of oil of poppies; ninety minutes by $\frac{1}{20}$, and a much longer time by $\frac{1}{10}$.

He recommends that, as it is difficult to place the oils always in the same conditions of temperature, and when exact results are wanted, that pure oil of olives should be tested with the reagent at the same time, and even mixtures of it and oil of poppies, in order that they may serve as a means of comparison.

I have repeated most of the experiments of M. Poutet, and find that the acid nitrate of mercury is a good practical test for ascertaining the adulterations of olive oil with oil of poppies and linseed oil; but cannot be considered so certain with regard to its falsification with the common rape oil of commerce. I find that rape oil is solidified by the acid nitrate of mercury in two or three days to the consistency of thick honey, and has a deep orange-brown colour; and M. F. Boudet states, that the oil extracted from the seeds of the Brassica campestris is solidified by the hyponitrous acid. In order, however, to ascertain the effect of a small adulteration, the following experiment was made. 10 grs. of rape oil were

* Annales de Chimie et de Physique, Aout 1832.

mixed with 90 grs. of Levant olive oil, and afterwards treated with $\frac{1}{12}$ of the acid nitrate of mercury, the whole was solidified in seven hours, but the consistency was much softer than that of pure olive oil treated in the same manner. I have treated many varieties of olive oil with the acid nitrate of mercury, and find that the consistency in general is nearly that of strong citrine ointment, but have never found any of them so soft as the portion that was falsified with rape oil; but this difference of consistency ought not to be depended on, unless a comparative experiment be made at the same time with pure oil of olives. Although both oil of almonds and castor oil are solidified by the acid nitrate of mercury, there is little probability of their being employed to falsify oil of olives, on account of their higher price. The appearance of the oil of almonds, when solidified by the acid nitrate of mercury, is very similar to the oil of olives treated by the same reagent, but it is much softer in consistency. Oil of almonds is also frequently adulterated, and I find that the acid nitrate of mercury may be employed to detect the presence of some foreign oils in it. 10 per cent. of rape oil, added to oil of almonds, prevents the complete solidification, and a small portion remains fluid, on the top, of a brown colour. 20 per cent. of oil of poppies, added to oil of almonds, prevents nearly altogether the solidification, the greater part remaining fluid of a brown colour.

Castor Oil is solidified very slowly by the acid nitrate of mercury, requiring two or three days to complete it; but, in the course of eight or ten days, it becomes as hard as pure oil of olives treated in the same manner, and has a peculiar odour. As neither M. Poutet nor M. F. Boudet have made any researches with regard to the action of the acid nitrate of mercury, or hyponitrous acid, upon fish-oils, I made the following, as well as the preceding experiments, in July and August 1834.

Dolphin Oil, of a pale citrine colour, and having nearly as little fish odour as sperm oil.—120 grs. of this oil were mixed with 10 grs. of the acid nitrate of mercury, in a half-ounce phial and frequently agitated, according to the method of M. Poutet. In a few minutes, it became of a milky colour, was

solidified in three hours, and, in twenty-four hours, was as hard and about the same colour as pure oil of olives treated in the same manner.

10 per cent. of rape oil, added to dolphin oil, and treated by the acid nitrate of mercury, renders it softer and more of an orange tint; but the indication is not sufficiently precise to render it of any use as a test, without a comparative experiment with pure dolphin oil.

Whale Oil, however, can be detected with more certainty. I find that 10 per cent. of whale oil, mixed with dolphin oil, prevents the complete solidification, and a portion of oil remains fluid, and the rest is more soft and brown, than pure dolphin oil treated in this manner.

Sperm Oil is solidified in from one to two days to the consistency of thick honey, by the acid nitrate of mercury, and is of an orange colour when the oil is pale, but when it is high coloured, it is rendered brownish.

20 grs. of rape oil were mixed with 80 grs. of pale coloured sperm oil, and treated with $\frac{1}{12}$ of the acid nitrate of mercury; its complete solidification was prevented, a very small portion remaining fluid, while the whole was rendered of a deep reddish-brown colour. Sperm oil is very frequently employed for greasing machinery, and when adulterated with rape oil, a practice which is exceedingly frequent, is productive of very injurious consequences. It is, therefore, a desideratum of great practical importance to detect this sophistication; but the acid nitrate of mercury cannot be considered sufficiently precise in its indications.

Whale Oil, Cod Oil, and Pale Seal Oil.—These oils were each separately treated in half-ounce phials with $\frac{1}{12}$ of the acid nitrate of mercury, and its operation on them all was found to be very similar, viz. that it causes the solidification of a small portion of the oil, which has a dirty orange colour, and which falls to the bottom of the phial, while the remainder retains its fluidity, and is rendered of a dark brownish-red colour. These last experiments were originally tried several years ago, with the view of discovering some indication for the discovery of the falsification of these oils with linseed oil, a practice which was carried on to a great extent in Glas-

gow at that period, and which rendered them inferior in the manufacture of soap. I found, however, that the acid nitrate of mercury gave no indication whatever; for its action on linseed oil is very similar to its action on these fish-oils, viz. whale, cod, and pale seal oils.

The following experiments were then made in order to discover some test for the presence of the ordinary commercial linseed oil. Equal bulks of alcohol of sp. gr. 0.815 and linseed oil were mixed together in a phial and well agitated for a few minutes, the alcohol separated in a short time from the oil, and acquired a greenish-yellow colour of considerable intensity. In order to ascertain if other fixed oils gave any indications of a similar nature, I submitted the following, procured from the most respectable sources, to the same treatment; viz. four varieties of olive oil, Gallipoli, Levant, Calabria, and Sicily, also rape oil, almond oil, sperm oil, dolphin oil, whale oil, cod oil, and pale seal oil. None of these, however, communicated any perceptible colour to the alcohol; but 10 per cent. of linseed oil, added to whale oil, gave a very perceptible greenish-yellow tint to the alcohol that was made to act upon it. This method I have used practically, in one or two instances, for detecting the adulteration of whale oil with linseed oil. It is proper to state, that the linseed oil employed in these experiments, was the brownish-yellow variety used universally for commercial purposes in Scotland, and which is always employed for the adulteration of fish-oils. There is also a pale yellow variety of linseed oil, which is very rarely to be met with, and which is only used in very small quantities, but which does not give the same precise indications with alcohol as the brownish-yellow oil.

I lately, however, met with a specimen of whale oil, represented to be genuine,* that communicated a slightly brownish tint to alcohol.

71 West Nile Street, Glasgow, 19*th April* 1838.

* It may be proper to state that the alcohol employed in the specimens sent to the Society of Arts, was rectified with sub-carbonate of potass without distillation, and that, in consequence, the oil has a milky appearance. In all the former experiments, the alcohol had been afterwards distilled.

*On the Decolorization of Palm Oil.** By WILLIAM DAVIDSON, M.D., Glasgow. Communicated by the Society of Arts of Scotland.

Palm Oil has long been used in small proportions in the formation of yellow soap; but, owing to the great quantity of colouring matter which it contains, it never can be used as a substitute for tallow even for yellow soap, unless it be deprived either partially or completely of its colouring matter. Nitric acid is employed by soap-makers for this purpose, and it has the effect of giving the oil a pale yellow colour; but, as soon as the acid which has been united with it is neutralised by the alkali, in the process of soap-making, its deep red colour returns. This method, therefore, is productive of no benefit. It becomes, then, a desideratum to decolorize palm oil in such a manner, as that its colour will not, in the least degree, return, on the addition of an alkali; for it has been completely established, by the experiments of soap-makers on the large scale, that palm oil forms with soda as good a soap as this alkali does with tallow. Chloride of lime possesses the property of completely depriving palm oil of its colour; but, unless the lime be separated from it, its properties are destroyed. This, however, may be readily accomplished by the following process; and, as the price of palm oil is frequently 10s. or more per cent. lower than tallow, the economy of using the former will be apparent.

Take from 7 lb. to 14 lb. of chloride of lime, and about twelve times the quantity of water. Triturate the chloride of lime in a mortar, or in an apparatus of a similar kind, gradually adding a portion of the water, so as at first to form a smooth and soft paste, and then the remainder, which will give the whole the consistence of cream. The object of this careful trituration is to break down every particle of the powder, so that it may be afterwards capable of intimate combination with the oil. Then

* Read before the Society of Arts for Scotland, 20th June 1838. The Honorary Silver Medal awarded 18th December 1839.

liquefy 112 lb. of palm oil, and, when the whole is melted, remove the fuel from the furnace, and pour in the solution of chloride of lime, and stir it carefully with a piece of wood, so that the whole may be intimately combined. Allow it to cool and harden ; then break it into as small fragments as possible, and expose it to the air and light for two or three weeks. After it has been thus exposed, put it into the vessel which shall be presently described ; add the same weight of sulphuric acid (previously diluted with about 20 parts of water), as of chloride of lime used, and boil with a moderate heat, until the oil drops clear from the wooden stirrer. When the boiling first commences, the liquid is liable to froth up, which must be moderated by constant stirring, and by the occasional addition of cold water. After the boiling has been continued for a sufficient length of time (which varies in proportion to the quantity operated upon), it may be allowed to cool, when the palm oil will remain on the top, and the acid liquor, with the insoluble sulphate of lime, at the bottom. In this process, more acid is directed than what is necessary to decompose completely the chloride of lime ; but an excess of acid facilitates the clearing of the oil, and it ought always to be preserved for a future operation; for, by adopting this plan, no loss will be sustained.

The vessel best adapted for the process is a cast-iron boiler lined with lead, placed over a common furnace ; for this metal is less liable to be acted on by the chlorine which is given off, and by the acid, than iron or copper. For the same reason, mortars made of these two latter metals should not be employed for the trituration of the chloride of lime with the water.

There is a considerable advantage gained by allowing the palm oil to remain in union with the chloride of lime for some time, before boiling them with the sulphuric acid ; for even a week makes a considerable change in the increase of decolorization. This may be easily explained. The chlorine of the chloride of lime gradually acts, in the one case, upon the oil, so as to deprive it of colour, and is at last wholly expended upon it; but, if boiling be immediately employed after the two have been united, a considerable quantity of the chlorine escapes

into the atmosphere. There is also considerable economy in combining the whole of the chloride of lime with the oil in the form of a cream, in place of making a clear solution of it; for, by the latter method, a considerable portion of the liquor does not unite thoroughly with the oil, and, consequently, the whole of its decolorizing power is not made available. Much more acid, however, is required, when the whole of the chloride of lime is combined with the oil, than when the clear solution is employed; but, as sulphuric acid is much cheaper than chloride of lime, an additional portion of it will be less expensive than an additional portion of the other. This process, with the largest quantity of chloride of lime stated, will render palm oil as white as ordinary commercial tallow, provided the oil and chloride have been intimately combined, and provided the mass has been exposed to the air and light for a week or two; and may be employed in the manufacture of white soap; but, where it is intended for yellow soap, the bleaching does not require to be so complete, consequently, much less of the chloride of lime and sulphuric acid will be required.

71 West Nile Street, Glasgow, 19*th April* 1838.

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*On the Removal of the Fetid Odours of Fish Oils.** By William Davidson, M.D., Glasgow.

The chief objection to the employment of the common fish-oils for burning in lamps and other purposes, is their disagreeable odour, and this is frequently not in proportion to their other qualities. Pale seal oil, for example, is, in general, very transparent, and burns well, although its odour is often almost insupportable. I noticed, in the *Edinburgh Journal of Science*, several years ago, a process by which the most putrid fish-oils may be deprived of their fetid odour; and several experiments which have been made, since that period. with dogfish-oil, &c. of an extremely putrid odour, have tended to confirm these results. The process is cheap, and consists of the use of the

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* Read before the Society of Arts for Scotland, 20th June 1838.

chloride of lime. This agent does not seem to possess the property of depriving, to any extent, fish-oils of their proper or natural odour; as, for example, it has little effect upon fine sperm oil, but its operation seems to be chiefly confined to the removal of the fetor which is the result of putrefaction. Hence the more fetid the oil, the greater will be the change effected by the process. The quantity of chloride of lime required varies according to the putridity of the oil; but, in general, 1 lb. of it is sufficient for 112 lb. of oil, but, if the oil be excessively putrid, 1½ or 2 lb. may be required. The process is the following:—Take 1 lb. of chloride of lime, and about twelve times the quantity of water. Triturate the chloride of lime in a mortar, or in an apparatus of a similar kind, gradually adding a portion of the water, so as at first to form a smooth and soft paste, and then the remainder, which will give the whole the consistence of cream. The object of this careful trituration is to break down every particle of the powder, so that it may be capable of intimate combination with the oil. Mix this thoroughly with the oil by frequent and careful stirring. Let them remain united for a few hours; then add 1 lb. of sulphuric acid, previously diluted with 20 or 30 parts of water, and boil, with a gentle heat, constantly stirring during the process, until the oil drops clear from the end of the piece of wood which may be employed for this purpose. After the boiling has been finished, allow the oil to settle for a few hours; then draw it off from the acidulated water. A common cast-iron boiler, lined with lead, is best adapted for this process, as being less acted on by the chloride of lime and acid; but a copper or iron vessel will answer the purpose, if the quantity of acid be not too great. For the trituration of the chloride of lime, however, mortars of iron or copper should be avoided. Fish-oil treated in this manner seems to burn equally well as the same oil which has not undergone this process; and a very ready and convenient method of trying it is with a floating light.

Glasgow, 19*th April* 1838.

*On the Removal of the Bitter Taste and Lichenous Odour of Iceland Moss, &c.** William Davidson, M.D. Communicated by the Society of Arts of Scotland.

Iceland moss *(Cetraria Icelandica)* is used, in some northern countries, in the formation of bread; but its extremely bitter taste has always been found a very great objection to it, as a general article of diet. M. Westring proposed the subcarbonate of potass as an agent capable of removing the bitter principle. He states that 32 grammes of the subcarbonate of potass suffice to produce the effect on 500 grammes of the lichen, steeped for twenty-four hours, in a quantity of water sufficient to cover it; or about one part of the alkali to sixteen of the lichen.

The time, however, which he directs for maceration does not appear to be sufficient to extract the whole of the bitter principle, and in order to ensure this (even with this proportion of potass) two or three days must be allowed. According to Berzelius, Iceland moss is composed of the following substances, in 100, viz. chloraphylle 1.6, bitter principle 3.0,† uncrystallizable sugar 3.6, gum 3.7, apothem of extractive 7.0, starch 44.6, bilichenates of potass and lime, &c. 1.9, amylaceous fibrin 36.2.

From this composition of the lichen, it is probable that the alkaline solution will take up the sugar, gum, and a portion of the extractive, but the starch and starchy fibrinous principle constituting about 81 per cent., will not be dissolved.

It therefore appears that a prolonged maceration of the lichen, in the solution of potass, will not cause a much great-

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* Read before the Society of Arts for Scotland, 20th June 1838. The Honorary Silver Medal awarded 18th December 1839.

† M. Herberger Pharmacian of Kaiserslautern, has lately obtained the bitter principle in a state of purity, which he has named Cetrarin. According to this chemist, it is white like magnesia, but he has not been able to procure it in a crystalline form. It has no odour but an intensely bitter taste. Alkaline solutions dissolve it very readily, but these combinations have a great tendency to be transformed into ulmates.—*Journal de Pharmacie, October* 1837.

er solution of the soluble matters than a shorter, for if the bitter principle be completely extracted, it is more than probable that the sugar and gum, as being readily soluble in watery liquids, will also be taken up. And in point of fact, on evaporating the alkaline liquid, after a fortnight's maceration, it seemed to contain very little solid matter.

In one experiment, however, I found the solution made with subcarbonate of potass, become gelatinous, which appeared to be owing to a partial degree of fermentation, for its bitterness was completely destroyed.

Caustic potass seems to have more effect in extracting the bitter principle than the subcarbonate of potass, a less quantity of the alkali being required. I have therefore generally adopted the following process.

Take 4 lb. of subcarbonate of potass (potashes) 4 lb. fresh quick-lime. Reduce the lime to powder, by sprinkling upon it, about a third of its weight of water, and put it and the potash into a jar along with 4 or 5 gallons of water. Cork the jar closely, and agitate it occasionally, for twenty-four hours; then decant the clear liquid, and pour it upon 112 lb. of Iceland moss placed in any convenient vessel. Add more water to the jar and agitate, so that the whole of the alkali may be exhausted. Add this also to the Iceland moss, and even an additional quantity of water, so that it may be completely covered. The maceration of the lichen must be continued for about a fortnight, but the time may be longer or shorter according to circumstances, and its state may be ascertained by taking out of the vessel, a small piece, washing and then chewing it for a minute or so. Straining the lichen by pressure is injurious, as tending to press out a part of the starchy principle, the water should therefore be simply allowed to drain off, before it be dried. As small a quantity of water as possible should be employed for dissolving the potass, for a strong solution has more power in extracting the bitter principle than a weak; it will therefore be found advantageous to divide the lichen into small pieces, as in that form it will have less bulk. The proportion of subcarbonate of potass recommended in this process is 1 to 28 of the Ice-

land moss, but if a more speedy effect is desired, the proportion of alkali must be increased.

Lime may also be employed for the extraction of the bitter principle. When Iceland moss is macerated with about a fifth or sixth part of its weight of quick-lime, with the requisite quantity of water to cover it, and frequent agitation employed, it is deprived of its bitter principle in about a week. The watery liquid acquires a deep brown colour, but it possesses scarcely any bitter taste, at the end of the maceration; the lime appearing to effect a partial decomposition of the bitter principle. Very little of the other soluble principles of the lichen seems to be dissolved, for on evaporating a portion of the solution, only a small residuum was obtained.

The Iceland moss itself is hardened in its texture, and assumes a reddish colour, from the action of the lime. The first quality, viz. the hardness, is completely removed by maceration in water acidulated with sulphuric acid, and the red colour is diminished, though not entirely removed by it. The following process may be adopted, if lime, as being cheaper than potass, be employed.

Take 112 lb. Iceland moss, 6 lb. recent quick-lime, and as much water as will cover them: macerate for about a week, frequently stirring during that time. After the maceration has been completed, throw the moss upon a sieve or drainer, having holes sufficiently large to allow the lime to pass, while the lichen is left; then pour a stream of cold water upon it, until it be thoroughly washed. The lichen is afterwards to be macerated in water acidulated with sulphuric acid, again washed with water without straining by pressure, and afterwards dried. Quick-lime also possesses the property of removing the lichenous odour, to a much greater extent than potass, and in this respect possesses some advantages.

I have tried Iceland moss, deprived of its bitter principle by caustic potass, in the formation of bread, by making a decoction of it (which, on cooling, formed a firm jelly), and mixing it with wheaten flour, and it formed a perfectly sweet bread, with a lichenous odour when warm, but which disappeared, to a great extent, when it was cold. The most eco-

nomical method of using the Iceland moss, in the formation of bread, would be to grind it, and afterwards mix it with one or other of the cereal grains; for by this method the whole of the amylaceous fibrin would be retained, which, though insoluble in water, must contain much nutriment.

The influence of chloride of lime on Iceland moss is also considerable. It deprives it, in a great measure, of its lichenous odour, and whitens it more or less according to the quantity of chloride employed, and according to the proportion of water used to dissolve it; for the same quantity of this agent dissolved in a small quantity of water will have a more powerful effect, than if dissolved in a larger quantity of the same liquid. The following method has been employed with the chloride of lime.

Take 100 parts of Iceland moss, divided into small pieces (previously deprived of its bitter principle) 12 parts of chloride of lime, and as much water as is sufficient to cover the lichen. Triturate the chloride of lime in a mortar with water to the consistency of cream, and then add it to the lichen. Macerate for a day or two, frequently agitating the materials; then wash with cold water, and finally macerate in water acidulated with sulphuric acid, and if any excess of acid has been employed, this must be removed by another washing with cold water. Iceland moss, acted on by this proportion of chloride of lime, is rendered pretty white, and forms a colourless and almost inodorous jelly when boiled with water. A less proportion of chloride of lime, will, however, in general answer the purpose, viz. 4 parts of it to 100 of Iceland moss. A similar effect is produced on Carragaheen or Irish moss *(Fucus crispus)* by chloride of lime; but it has more effect in destroying its odour, than in bleaching it.

Glasgow, 17*th April* 1838.

*Report of the Experimental Committee of the Society of Arts for Scotland, on Dr Davidson's proposed methods of Decolorizing Palm Oil, Removing the Bitter Taste and Lichenous Odour of Iceland Moss, and the Fetid Odour of Fish Oils, and for detecting the Adulteration of Fixed Oils.** Communicated by the Society of Arts of Scotland.

I.—*Decolorizing Palm Oil.* (555.)

The method proposed by Dr Davidson was tried with a palm oil, very dark in its colour, and succeeded completely in bleaching it to that degree that it may be used in the manufacture of white soap.

A specimen of the oil before and after being bleached is sent.

I can conceive only one objection to the successful practical application of this method. The chloride of lime, when first applied, decolorizes to a certain extent; and the bleaching seems to be afterwards completed by exposure to light; hence the absolute necessity of breaking the congealed oil to pieces, and frequently stirring it, so as to expose fresh surfaces to the light. When this is not done, the decolorizing process goes on slowly. With a little oil, the bleaching may be effected in a day or two,—of course the time depends on the state of the weather: with sunshine, it goes on rapidly. When the quantity is very large, I suspect it will take a much longer time to complete it than Dr Davidson mentions; but this is mere conjecture. The quantity on which I operated did not exceed a quarter of a pound, which required nearly a fortnight, with very frequent stirring. The weather, however, with the exception of the three last days, was unfavourable, there being little sunshine.

If this objection does not apply when the process is conducted on a large scale, I consider the method proposed well adapted for accomplishing the object in view.

II.—*On the Removal of the Bitter Taste and Lichenous Odour of Iceland Moss, &c.* (557.)

Two processes are recommended by Dr Davidson: one by the use of solution of potassa; the other by lime. Both of

* Read and approved 10th April 1839.

these processes were tried and found to succeed; the bitter taste being almost, if not altogether, removed from Iceland moss; and after removal of the taste, the bleaching process was applied, and the colour, though not entirely, yet was very much discharged.

III. *On the Removal of the Fetid Odour of Fish Oils.* (556).

This process consists also in the use of chloride of lime. It was tried, as recommended, on two samples of oils mentioned by Dr Davidson, viz. *seal oil* and *whale oil.* Though these were affected by the chloride, yet they were by no means deprived of their odour. Dr D. states that chloride has no effect on the *proper* or *natural odour* of the oils, but seems to be confined chiefly to the removal of the fetor, which is the result of putrefaction. Whether the odour of the oils tried was caused by putrefaction or not I cannot say, but certainly the odour was little altered by the chloride. Supposing the chloride to remove the odour, there is an objection to its use, but which may perhaps by experience be overcome. I allude to the action of the lime on the oil, and the formation of soap, or *carron oil* as it is commonly called; which not only wastes the oil, but renders it difficult to separate it from the impurities. Shaking with water after the mixture has been boiled, as recommended, with oil of vitriol much diluted, causes, after rest, the subsidence of sulphate of lime, but still the oil continues milky. I found it necessary to filter the oil through bibulous paper, so as to get it of its original transparency.

Samples of seal oil and whale oil submitted to the process are sent.

IV. *On the Adulteration of Fixed Oils.* (554).

In investigating this subject two objects were kept in view. 1st, Whether the test proposed is a good one; and 2d, supposing it to be good, whether means might be adopted to prevent it from acting as a test. The test proposed, which applies solely to linseed oil, when used as the means of adulteration, is alcohol of specific gravity 815; which, according to Dr Davidson, acquires a yellowish-green tinge when shaken with the oil. I have found that this is the case, and that as

stated, the alcohol is not tinged when shaken with other oils, such as seal oil, whale oil, &c., that one-tenth part of linseed oil mixed with the others, gives to alcohol the yellowish tinge; so that we may consider the test as an indication of the presence of linseed oil, even though in small quantity; but, though under certain circumstances it acts as a test, we cannot put confidence in it, for if the oil be previously mixed with a little chloride of lime, as recommended by Dr Davidson in the decolorizing of palm oil, though the colour is not destroyed, yet the alcohol acquires no tinge from it. When oil thus treated with chloride was mixed in the proportion of one-fourth and of one-third with other oils, the mixture did not tinge the alcohol, indeed the oil itself when shaken with alcohol did not communicate colour to it. Though therefore, when an oil is shaken with alcohol, and communicates to it a yellowish-green tinge, we may conclude that it is adulterated; yet we are by no means warranted in concluding that there is no adulteration when the alcohol is not tinged.

Samples of oils, pure and adulterated with linseed oil, before and after admixture with chloride of lime are sent.

A. Fyfe, *Convener.*

Edinburgh, 11*th March* 1839.

List of Specimens referred to in the Report.

1. Palm oil.
2. Palm oil bleached.
3. Iceland moss.
4. Iceland moss treated with potassa.
5. Do. with lime.
6. Do. with potassa and thin chlorid lime.
7. Seal oil shaken with alcohol.
8. Linseed oil do.
9. Whale + $\frac{1}{10}$ linseed oil + alcohol.
10. Linseed oil + chlorid lime + alcohol.
11. Seal oil + $\frac{1}{4}$ linseed of oil of No. 10 + alcohol.
12. Whale oil.
13. Seal oil.
14. Whale oil + chlorid lime + sulphuric acid.
15. Seal oil do. do.
16. Linseed oil.
17. Do. + chlorid lime.

Approved by Experimental Committee, adopted, and forwarded to Society.

20*th March* 1839. John Scott Russell, *Pres.*

Some Remarks on Electric Currents in Metalliferous Veins. By Robert Were Fox, Esq. Communicated by the Author.

I have been much interested by Professor Reich's report of his researches on the electrical currents in metalliferous veins, inserted in the last number of the Edinburgh New Philosophical Journal; and it is satisfactory to me to observe, that his results are so much in accordance with my own. It is true, that I did not obtain, as he has done, evidences of such currents on connecting ore-points with *non-metalliferous rocks*, but this was, doubtless, owing to the inferior delicacy of my galvanometer.* Notwithstanding this circumstance, it was sometimes acted upon very powerfully when two ore-points were connected, far more so than it could have been by the reciprocal influence of the copper plates, however near together they might have been placed, and yet they were, in some instances, from 300 to 400 yards apart. Moreover, when a *zinc* plate was substituted for a copper plate, first at one of the ore-points, and then at the opposite one, the direction and intensity of the currents were unaffected by the change;* whereas they would have been in opposite directions had they been generated by the mutual action of the plates; their intensity would also have been increased, and so it would, had they arisen from an action between an ore-point, and one of the plates. Besides, it should be recollected that the plates could not have had any action whatever on each other when in *contact* with ore-points, which were *good conductors* of electricity, as they almost always were; and when the *points* only of the copper wires were employed instead of the *plates*, for making the contact with the ores, the results in both cases were the same. In one mine, electric currents produced by a pair of large plates of copper and zinc plunged into strong brine, were made to traverse the long wires, and counteract those derived from the vein itself. In those experiments it

* A description of my galvanometer is given in Philosophical Transactions 1830, in note, p. 399.

† See Fourth Report of the British Association for the Advancement of Science, 1834, p. 573.

appeared, that, in one instance, the artificial currents predominated; but in others, in which the ore-points were further asunder, the opposing currents either neutralized each other; or the *natural* ones were only *diminished*, not reversed.*

In my earlier experiments, I used copper wires coated with a solution of sealing wax in alcohol;† but this protection not having been found necessary, was afterwards dispensed with.

I have been induced to recapitulate the tests and precautions which I adopted, because I conceive, that had they not been overlooked in some quarters, no suspicion could have existed that the electricity observed in mines, might have arisen from the apparatus employed, or at least that it was not fully proved to have been derived from the veins themselves. It is satisfactory to find that Professor Reich used similar tests, and that he deems them to have been as decisive on this point as I have done.

I quite concur with him in considering the phenomena in question to be *hydro-electric*, and this opinion I have long maintained,‡ for the indications of electricity in the veins seemed to depend on the existence of imperfect conductors between the masses of ore; and, moreover, it is found that the different ores, when connected, produce electric currents even in rain-water, and in water taken from the mines, the action is still more considerable. Professor Reich has stated the order in which he found some of the ores to follow each other in their voltaic relations. His series differs very slightly from the one which I have given in the Philosophical Transactions for 1835, p. 39, owing doubtless, to some differences in the specimens which we used in our experiments. Thus I find that different specimens of copper-pyrites, iron-pyrites, arsenical pyrites, &c., vary much in these relations with respect to each other; and ores of the same name; two pieces of copper-pyrites, for example, in rain-water, will often cause considerable deviations of a sensible needle. Some ores which were partly

* See Fourth Report of the British Association, 1834, p. 573.

† Phil. Transactions, 1830, p. 399.

‡ Phil. Transactions, 1835, p. 39. Also Fourth Report of the Cornwall Polytechnic Society 1836, pp. 127-128; or Sturgeon's Annals of Electricity.

immersed in rain-water for more than a fortnight, and then compared, may be classed in the following order:

+ Zinc,	+ Sulphuret of Silver,
Copper,	Copper-Pyrites,*
Copper Nickel,	Iron-Pyrites,*
Vitreous Copper,	Arsenical Pyrites,*
Purple Copper,	Loadstone,
Galena,	Peroxide of Manganese.

My experiments in mines appeared to indicate a general tendency in the electric currents to take a westerly direction, in veins inclining or dipping towards the north, and an easterly one, in those dipping towards the south;† whilst downward currents were mostly obtained where masses of ore at different depths were connected. It is true that such results were not without exceptions; and W. J. Henwood's researches in mines, as well as those of Professor Reich, would seem to render it premature, at least, to form any general conclusions on the subject. Local causes may, however, often supersede those of a more general character; thus, for example, if a mass of copper-pyrites, or iron-pyrites, were connected with another of vitreous copper, or galena, the currents would probably pass through the apparatus towards the latter, especially if not very distant from the former; and even differences in ores of the same name may more or less influence the directions of the currents where small insulated portions of ore are connected, whereas, where the masses of ore are large and far asunder, the more general law, if such there be, may predominate.

This point will not probably be decided without more extended researches in different mining districts; and these, it may be hoped, will be undertaken, not for this reason only, but also for the purpose of determining whether the electro-

* These three * * * differ very little from each other, or from platina or palladium, in their electrical relations, and they often change places, especially iron and arsenical pyrites.

† It may be remarked, that, in this latitude, veins, having an inclination towards the south of 40° from the vertical, are parallel to the axis of the earth; and they are nearly perpendicular to it, when they have the same degree of inclination towards the south. It is true that veins have seldom so great an inclination as this in Cornwall, but very commonly as much as 30°.

magnetic apparatus can or cannot be rendered useful in practical mining. I have before now thrown out the suggestion; and the probability that it may be so applied with advantage, appears to be much increased by some of Professor Reich's experiments, already referred to, in which he obtained electric currents when an ore-point and a non-metalliferous rock were connected. Some of these currents traversed the wires from the rock towards the ore-point, and therefore could not have been produced by any action between the latter, and the plate at the former. Even if ore existed behind the rock, its action on the metallic plate in contact with the rock, would have tended to produce a current towards it from the ore-point at the first station, unless counteracted; as may be inferred, by a greater action, in a contrary direction, between the masses of ore near the opposite stations.

These are questions of practical importance, and only to be solved by more experiments, which may be easily made by miners themselves after an hour's instruction in the use of the apparatus. Thus a metallic connection having been established between a mass of conducting ore in a vein and the galvanometer, by means of a copper plate and wire, the plate attached to the second wire may be carried through a level or gallery, and pressed for an instant against the locks on each side, at intervals of a few yards, the other end of the wire having been first connected with the galvanometer. If, when contact is made with the rock at any part of the level, the galvanometer should be particularly affected, the experiment should be repeated, till the part of the rock is discovered at which the maximum effect is produced. A zinc plate may then be substituted for the copper plate, or pressed between the latter and the rock; and, if convenient, platina or palladium may also be used for making the contact; and the modifications of the currents produced by the different metals should be noted. With one of the two last-named metals they are most likely to be exhibited as they exist in the vein, unaffected by the apparatus. The same tests may be applied to any water flowing into the level, in order to ascertain whether or not it is connected with ore in the neighbourhood.

Observations on the Digestive Organs of the Infusoria.

By J. Meyen.

All naturalists are aware that Gleichen, in 1781, tried to make certain infusoria eat carmine, and observed next day that they had several large red granules in the interior of their bodies. He thence concluded that they had swallowed the colouring matter. He likewise noticed that these coloured granules afterwards made their escape by another opening. Gleichen has figured these red granules very accurately; each of them is in the centre of a particular circle, the nature of which he does not explain. At a later period M. Ehrenberg made the same remark, and he thence concludes that the infusoria have several stomachs, which in one section are destitute of an intestinal canal, while in others they not only possess canals by which they communicate with each other, but lateral appendages besides, which terminate in a cœcum. In consequence of these discoveries, these infusoria were designated by the name of Polygastric Animals. M. Ehrenberg believes that he has proved that their stomachs are filled one after another, and he has figured, more or less completely, the intestines which form the communication between the different stomachs.

Many observers have already questioned these assertions of M. Ehrenberg;* for my own part I never admitted them, because, in the first place, I never could see the intestines which form the communication between the stomachs, and likewise because I have observed, many years since, that these supposed stomachs were moving in the interior of the body of many species with great rapidity, in the same manner as the granules which circulate in the joints of the *chara*. I have often seen Vorticelles with nine or ten large globules of indigo in the belly, which always moved round a centre, and thus shewed, in the most evident manner, that they could not have a communicating canal between the stomachs provided with an anal orifice and an extremity directed to the mouth.

* See the Memoir of M. Dujardin on this subject, in the 10th volume of the Annales des Sciences Naturelles.

But, it will be asked, what are these vesicles and balls of the same diameter, existing in the bodies of the infusoria, and which have been taken for stomachs? This question I have continued to ask myself, till an attentive and long continued investigation has enlightened me as to their origin.

The true infusoria are vesicular beings, whose interior is filled with a mucous substance; the thickness of the membrane forming the vesicle can easily be ascertained in some of these animals, and in many species I have noticed in this membrane an obvious spiral structure, which establishes a complete analogy between it and cellular vegetables. In the large infusoria, a cylindrical canal (the œsophagus) obliquely traverses the membrane which forms the animal. The lower extremity of this canal dilates more or less when the animal has taken food, even till it attain the dimensions of the balls, which are found in the interior of these same infusoria. The inner surface of this part of the intestinal canal is provided with ciliæ, which turn round not only the alimentary substances, but also foreign bodies, till they have acquired a spherical form. During the formation of this ball the stomach (for it is evident that we must distinguish this organ by that name) has a free communication with the œsophagus, and, by means of the ciliary apparatus found at the exterior, new alimentary substances are introduced into this canal, and pushed as far as the stomach; but I could not satisfy myself whether the œsophagus was likewise beset with ciliæ in the part which separates the stomach from the buccal orifice. When the ball has acquired the size of the stomach, it is expelled by its other extremity, and pushed into the cavity of the animal. It then forms a new ball, if any solid substances exist in the surrounding liquid. This second ball is itself pushed into the interior of the cavity of the animal, and drives before it the first ball, along with the mucosities between the two; the successive formation of similar balls by the matter received into the animal, continues in the same manner without interruption. It is the simultaneous existence of many of these balls that made M. Ehrenberg believe that these animals were polygastric. If solid substances do not exist in the surrounding liquid, then the balls are less solid, and they appear in the forms

which they present in the infusoria plunged in colourless liquids. In this case the balls are composed of a small number of particles, and principally of a considerable mucous mass which unites them. Sometimes two balls of this kind are so pressed against each other by the contractions of the animal, that they at last unite.

If we wish to follow the formation of these balls, it is necessary to commence these observations at the moment when the infusoria are plunged into the coloured liquid. The deglutition of the coloured particles takes place very quickly, often in about half a minute, and the coloured balls issue, one after another, from the stomach, and are pushed downwards along the internal wall of the cavity of the animal. In the Paramæcies, Keronies, and Vorticelles, the new ball pushes the preceding before it along with the mucosities between them, in such a manner that the first rises along the opposite wall, returns to the other extremity of the cavity, and is pushed downwards on the other side. The balls thus accumulate in succession, till they are expelled, one after the other by the anus. The number of these balls is often so considerable as to fill the whole cavity of the animal, and so close together, that they form a large mass, which turns slowly upon itself, as among the Vorticelles.

This rotation is the result of the force with which the newly formed ball is pushed from the stomach into the cavity, and moves along the under side of the preceding ball. In other cases, where there are not yet many balls, we likewise remark the circular rotation alluded to, but I cannot in this instance say what is the cause of it.

Thus, in the true infusoria, the substances which they absorb are introduced into the abdominal cavity in the form of balls, and from these the stomach extracts the nutritive substances. The residue remains in these same balls, the mucosities interposed are reabsorbed, and even in the interior of the stomach the particles of the ball are disintegrated, although this happens but seldom.

What is the nature of those vesicular cavities, of such great numbers, and so variable in size, which appear in the interior of the infusoria? They are not stomachs; they possess no-

thing in common with the balls of which we have spoken, although the latter may get into them singly; but this can only be considered as accidental. We may trace the formation of these cavities, and perceive their sudden and complete disappearance, with as much ease as the formation of the balls. Nay more, it is sometimes possible to see how one of these cavities moulds itself over a ball, and speedily afterwards disappears. The microscope shews that these cavities are not lined with a particular membrane, but are mere excavations of the pulpy substance. They likewise often appear very near the inner surface of the membrane which forms the skin of the animal, and some of them increase to such a size that their diameter is equal to the third or the half of that of the entire cavity of the infusoria. The slight refraction which the rays of light undergo at their circumference, proves that these cavities are not filled with air, but by a liquid; and in the large infusoria, it is easy to satisfy ourselves that they do not open on the exterior. Similar cavities are formed in the mucus of true cellular plants, particularly in certain aquatic Cryptogamia.

My botanical labours prevent me from carrying these researches farther; but enough has been said to induce the naturalist to pursue them. They require a great degree of perseverance, for it is not easy to establish these facts in all infusoria; but they are of high importance, since the order Polygastria has already been admitted into many modern treatises on Zoology.

Researches on the Structure of the Scales of Fishes. By Dr L. Mandl. With an illustrative Plate. Continued from page 126 of this volume.

We have learned from the first chapter, which gives a historical sketch of the researches which have been made on tegumentary appendages, that all authors agree in considering these tissues as produced by a secretion, and formed by homogeneous layers, similar to those observed in the shells of bi-

valves. This opinion, first expressed by Leuwenhoek, and adopted without modification by all subsequent authors, rejects therefore entirely the idea of an internal nutrition, or true organisation, which would have exhibited in a scale a tissue receiving and conducting nutritive substances, and undergoing various degrees of development.

Now, the result of our researches on the structure of scales precisely proves the existence of this organisation, which has escaped the observation of the authors alluded to. If we ask the cause of this, we will readily find it in the insufficiency of the means they employed. No one had made use of a compound microscope, nor any considerable magnifying power; scales were examined with the aid of a glass, magnifying from five to ten times at the most, and the comparative study of these appendages of the skin in different families, and in the successive degrees of their development, was entirely neglected.

We may therefore say, without hesitation, that we have been the first to employ more effective means of investigation. The magnifying power used in studying the internal structure was always three hundred times; and it is on this scale that the drawings for the illustrative plate were executed.

We have by this means proved that the greater part of scales are composed of two layers lying one above the other; the lower presenting the structure of fibrous cartilages, the upper that of corpuscular cartilages; the latter, besides, shewing a variety of lines, which we shall prove to originate in the fusion of the primitive cells. These two layers are traversed by longitudinal lines, which belong to both of them.

We shall treat of each of these particulars by itself, affording in the first place an idea more in detail, of a scale as it appears under a less magnifying power; for example, a hundred and fifty times.

Let us take as a specimen a well developed scale; that of the carp for example. (Pl. III. Fig. 2). We observe upon it longitudinal lines, tending from a common point towards the circumference, the number being not so considerable but that they may easily be counted. The place where these lines converge, or at least to which they are directed, is of greater or less size; this we call the focus. Between the longitudinal lines, and more or less parallel with the margins, a very considerable number of lines may be observed, which are interrupted by the longitudinal lines, and which either

anastomose with each other, or are continued without interruption; these are the cellular lines, for they have their origin in the cells. Finally, a kind of spines exists on a great number of scales, on one of their edges, which we call the teeth of the scales, a denomination founded on the development of these bodies.

Around the longitudinal and transverse lines, principally towards the points where the longitudinal lines converge, viz. the focus, we find some yellowish corpuscles, more or less elliptical, which we name the corpuscles of the scales; and lastly, upon tearing away or raising the upper layer of the scale, an inferior fibrous layer makes its appearance.

Each of these formations will be specially treated of; but, in order to facilitate description, it will be advisable to divide the scales into several parts. Let us suppose lines drawn from the four corners of the scale, as we have already seen more or less marked in nature—(pl. III. fig. 2)—across the point of convergence of the longitudinal lines; the scale will then be divided into four quarters or fields. We name (and have drawn the scales conformably to these denominations) that portion which is implanted in the skin, and directed towards the head, the *basal field*; that opposite to it, directed towards the tail, and which, from its imbricated position, is free, and not covered by the other scales, the *terminal field*. As a consequence of these denominations, the *right field* lies next the dorsal region, and the *left field* towards the ventral region. These two last are almost always identical, and we name them the *lateral fields*.

The scales of carp, and of some other fishes, distinctly present this division into four parts; and it is easy to recognise the regions indicated in every scale.

Longitudinal Canals.—The longitudinal lines which run from a common point, the focus of the scale, towards the periphery, are of great importance in the anatomy of the tissue we are now examining. Hitherto no opinion has been expressed regarding them, because at first they were not examined, and subsequently because authors, perceiving in the cellular lines the limits of continual increase, hastily passed over in silence an appearance which they could not explain, and which likewise threatened to be strongly opposed to the views they were advocating.

If it so be that the scale, as was first stated by Leuwenhoek, and as all authors have repeated up to the present time, is composed only of superimposed layers similar to each other, and whose edges are marked by these more or less concentric lines which we call the cellular lines, how are we to explain those broad striæ which issue from a common centre, and which, by interrupting the peripheral lines, often separate the latter by wide and vacant spaces? How comes it to pass that such broad bands, amounting to ten, twenty, or even more, exist in a scale, and are not marked by the lines which denote the continual growth of it?

Authors have thus been in general contented by calling these lines furrows, diverging in the form of a fan, &c. By the examination to which we have subjected them, we have succeeded not only in determining the

different forms of these lines, and the modifications they undergo in the series of scales, but have likewise illustrated the functions which they probably perform in the organization of the latter.

These longitudinal lines undergo every degree of formation, from that of a perfectly closed canal, to that of a simple furrow. We shall sometimes see them present the form of a canal pierced on its upper side, permitting a view of the thickness of the walls so pierced, as well as the bottom; sometimes these same lines will appear in the form of canals with the whole of the upper wall removed; sometimes they are merely furrows, placed in the upper layer of the scale, and having the lower bed of this tissue as a base. In certain cases we perceive them traversing the osseous scale singly; at other times they anastomose with each other, and scales are to be found on which they form spines. We shall now proceed to study these forms in detail.

If we examine the scales of *Acerina vulgaris*—(pl. III. fig. 4)—it will be seen that the longitudinal lines appear as canals alternately contracted and dilated. The contracted parts present nothing but a rounded body; and nothing throws light either on its nature or structure. We should remain in uncertainty, if the dilated parts, which are a continuation of the same lines, did not assist us in discovering their organization. The dilated parts evidently shew that the upper wall is partly removed; we then see not only the bottom of the canal—(fig 4, *a*,)—which is sometimes granular, at other times presenting transverse striæ; but we can generally, likewise, distinctly perceive the thickness of the wall indicated. It is bordered by a double line (fig. 4, *b*), one of which belongs to the opening. Cases also exist in which the opening is indicated only by a simple edge. These different forms may be explained by the different degrees of inclination in which the upper wall has been removed; sometimes, in fact, it is merely grazed, while at other times it is more or less deeply cut.

If we now proceed to examine a scale of *Mullus barbatus* (pl. III. fig. 5), we shall still perceive longitudinal lines closed at intervals; but the upper wall is removed in considerable portions, and the bottom of the canal easily seen. These are, therefore, hollow tubes, the upper wall of which is wanting throughout the greater part of their course. The bottom is placed on the under layer of the scale, and the lateral walls are formed by the upper layer. The engraving sufficiently shews, that the cellular lines are never continued into the closed canals, neither in the latter case, nor in the scale formerly described.

The scale of *Serranus* (pl. III. fig. 6), presents in its longitudinal lines hollow tubes, of which a part of the upper wall is entirely wanting. We perceive from the engraving that the canal is nowhere closed; but it is easy to distinguish the bottom as well as the lateral walls, which are covered with granulations, of which we will afterwards find an explanation. The longitudinal line, in this instance, still approaches more or less to the rounded form of a canal; it is an intermediate stage between a canal and a furrow.

Lastly, if we examine a scale of *Percarina Demidoffii* (pl. III. fig. 7), and

this observation may be repeated on a multitude of other scales (for example, the membranous ones), we will find the canal now transformed into a true furrow. The upper wall is entirely wanting; the lower wall, or rather the bottom, is formed only by the inferior layer of the scale (fig. 7, *a*), which constitutes the marginal space by its prolongation. There are no lateral walls properly so called, they are only formed by the interruption of the superior layer.

In this case, therefore, it is not a hollow tube, as in the preceding examples, which traverses the scale, but it is simply the absence of the superior layer which constitutes the longitudinal line. By glancing at the figures from 4 to 7, we have the different forms under our eye, from a simple furrow to a tube almost entirely closed, or pierced only in several places.

This last example demonstrat s that the canals, or rather the tubes by which more or less osseous scales are traversed, are of the same nature as the longitudinal lines of which we have formerly spoken.

In fact, if we examine the plates which cover the surface of *Syngnathus*, we shall find them pervaded by tubes entirely closed, in no instance destitute of walls, insulated towards the margin, and anastomosing towards the centre of the scale; or enclosed, towards the bottom, between the anastomoses of these tubes, forming numerous insulated compartments.

This tissue does not present an osseous structure; but in scales characterized by this latter organization, such as that of the *Scinci* (pl. III. fig. 8), the longitudinal lines form tubes perfectly closed and rounded as in the preceding example. The ground exhibits osseous corpuscles with small canals attached to them (fig. 8, *b*). All osseous scales, plates, &c. display nearly the same form, a circumstance which has caused us to limit our observations to the scales of the Acanthopterygians and Malacopterygians, the scales of other fishes, as far as we have had an opportunity of observing them, possessing only closed tubes traversing an osseous tissue.

These tubes sometimes rise above the surface of the scale and form true spines, as in *Gadus euxinus*, skate, &c. They then appear to be composed of a fibrous tissue, as indicated by the longitudinal lines which run from their base towards the summit.

By casting our eye over these different forms presented to us in the study of the scales mentioned, and which might be further increased by many examples of intermediate degrees, it seems to us proved that the longitudinal lines appear under all forms, from that of a simple furrow, to that of a closed and hollow tube, which latter is found filled in osseous scales. But in the majority of cases, this tube is hollow, and, whether it be closed or open, it may always be considered as a canal. Elementary anatomy shews us that osseous tissues are at first cartilaginous and traversed by vessels, hollow tubes, which, at a later period, become filled up. Thus, the tubes even of osseous scales (pl. III. fig. 8), take their place by the side of the others, if we examine them during their development, before they have attained the highest stage of their organization.

The lines in question, then, present us in all their forms, with a series

of hollow tubes, which may be truly called canals. These canals traverse the scale longitudinally; they tend towards a focus which, as we shall afterwards shew, is a centre of nutrition, a place where the tissue is found in its development. Such of them as are situate in the basal field, are in direct relation with the skin; they do not exist in the terminal and lateral fields but when these are covered by the skin. Would it be too rash, then, to infer from these premises, that the canals serve to transport the nutritive juices of the skin towards the centre of nutrition and development, and that they have an intimate connection with the organization of the scale? At the same time, we wish to advance nothing before having confirmed this opinion, by the study of scales in their development; but to us, there appears, even at this stage of our investigation, no doubt that these canals perform the functions of true nutritious vessels.

The longitudinal lines do not always reach the focus, and after leaving that point, they do not always extend to the periphery.

Cellular lines.—The greatest difficulty we have experienced in studying the intimate structure of scales, was the explanation of the numerous lines which run along them in a direction more or less parallel to their edges. We cannot, for many reasons, admit of the explanation given by authors, who have seen nothing more in them than the edges of the secreted layers.

A single microscopical inspection refutes this opinion. If we examine one of the scales, even the most perfectly formed of *Corvina nigra*, (pl. III. fig. 9), for example, we perceive that these lines rise above the surface, and that they are placed on a separate base, which is different from the under layer of the scale, and the corpuscles.

These lines are very short, and as it were divided into small pieces, in the middle of the scale; they are interrupted by the longitudinal vessels. We often find, in scales preserved among spirits of wine, large spaces deprived of these striæ; by rubbing the surface, it is easy to remove any quantity. How can we find in all these circumstances, any thing to favour the opinion that these lines are the edges of the homogeneous layers of the scale? How can we fail, on the contrary, to regard them as subversive of the notion hitherto held by authors?

We ought, therefore, at the very outset, to reject these ideas, and seek for an explanation in the comparative study of scales. This we have found by observing scales which may be said to be membranous, and much less advanced in development and complication. By these means, we have arrived at the certainty that these lines owe their origin to the cells which, in the first instance, are formed in the upper layer of the scale, placed on a base; that, by slow degrees, these cells become filled, elongated, and at last represent lines of variable width, which, at the most, by an unequal edge, reveal their primitive nature. We shall now give examples.

Let us first examine the scales of *Ophidium barbatum*, which are very

nearly of the form represented at fig. 3 of the plate. We perceive longitudinal canals emanating from the focus, and between these concentric series of lines which we shall observe more in detail. This scale, which is magnified three hundred times, shews that the concentric lines are composed of insulated cells, each of which presents the cell properly so called, and its base. It is separated from the neighbouring cells by a narrow furrow, so that each cell with its base rests insulated on the base of the next cell. Longitudinal canals are likewise observable: sometimes the posterior edge of the base is distinct; each cell presents on one side the thickness of its wall indicated by a double line.

In *Gadus euxinus*, the cells are still insulated, but the bases are already united, and extend in parallel plates to the longitudinal canals; we perceive distinctly the two layers of which the scales are composed; the lower one forms the bottom of the canals, the upper one the base of the cells.

The *Eel* presents one degree of further development; the cells form lines which become more distinct around the focus; many series only are separated by a wide space; the bases of the insulated cells are all united in a uniform layer; the cells are surrounded by a double line.

The cells on the surface of the scales of *Motella tricirrata* (pl. III. fig 10), are a kind of bubbles disposed in series, and prominent, so that they can be made to lie over on either side we may wish. The upper layer is divided only by the longitudinal canals.

We have represented (pl. III. fig. 11) a portion of the scale of *Mullus barbatus*, taken from the lobe between two longitudinal canals; we see at the same time, the composite series of the insulated cells, and others, which permit the view of the cells which compose them; these latter begin to be confounded, only the exterior edges remaining, which at last disappear.

Serranus scriba (pl. III. fig. 12) affords a very remarkable example of the origin of these lines by the fusion of the cells. We see very distinctly an edge of the base occupied by globular cells, very minute, at first well marked, but gradually diminishing, till at last they form only a crenulated line.

The carp has scarcely any cells in its lines, and the greater part of the latter appear in a form which we shall examine afterwards.

In the series of scales which we have successively noticed, we have had examples of cells sometimes elevated on one side only, indicating their thickness by a double line; sometimes elevated throughout their whole circumference, and which can be made to fall either on one side or another, but always placed on a separate base, which is very distinct from the inferior layer of the scale, and which, in common with the cells and corpuscles disposed in it, form the superior layer.

We have likewise followed, in this series of scales, their successive fusion, that is to say, their union with each other in order to form the lines which we have named cellular lines, and which, without this preli-

minary examination, would have been altogether unintelligible. But we are now in a condition, on looking at any scale whatever, that of *Corvina nigra* (fig. 9), for example, to understand the organization of these lines; we perceive that the lines derive their origin from the cells; one of their edges is still irregularly undulated; they are prominent, raised above the scale, and form the most salient points of it. We perceive beneath the bases of these lines deeper than the rest of the scale, and placed lower than the lines. Of this we may easily convince ourselves by the microscope, because, in order to get a good view of them, it is necessary to change the focus, although sometimes only very little. With regard to the corpuscles, we shall speak of them afterwards; merely remarking, in the mean time, that, in the engraving alluded to, the scale is presented in the position where the lines are in the focus, which prevents the corpuscles from being distinctly seen.

If we examine these lines near the terminal field, in scales which are provided with teeth, we perceive them often joined to the neighbouring line by a curvature, which gives them the appearance of a line forming the continuation of the other which is curved. We shall afterwards speak of the form of these lines in the focus.

We have thus arrived at a result very different from that come to by those observers who consider these lines as nothing but the edges of the layers of the scale. Such an opinion, which the slightest microscopical inspection would oppose, for the reasons we have mentioned, is entirely overthrown by our observations. We should have preferred following the different degrees of the development of the scale in the same individual; the comparative study of them, however, has furnished such satisfactory results, that we may hope it will supply the researches indicated.

The Corpuscles.—We know that cartilages are provided with corpuscles of different forms, and which are characteristic of that tissue; a perfectly analogous formation is found in scales. These corpuscles are yellowish, the colour more or less deep, of an oblong form, more or less elliptical. They are seen very distinctly at fig. 13, pl. III. Near the edge of the scale they diminish in size, and at last form only a kind of granulations. The same thing is likewise remarked occasionally round the longitudinal canals.

When examined with a higher magnifying power, their shape can be more distinctly observed; we perceive that their sides are sometimes a little dilated, that their rounded ends are at other times united to very minute corpuscles, or terminate irregularly. They become very pale, and form large plates in the focus. Their ordinary length is about the hundredth part of a millimeter, but, as already mentioned, their size is very variable. These corpuscles appear to be disposed in very regular series, which, however, we have not hitherto succeeded in perceiving distinctly; they sometimes lie across each other so as to form a kind of cross. They seem to us to perform the same function as the corpuscles of bones and cartilages. Acids render them transparent; and, according to the account given of the corpuscles of bones, they should contain salts. If we place the scale, for example, in hydrochloric acid and leave it there for

some time, the corpuscles almost entirely disappear, and salts are deposite.

These corpuscles are placed in the tissue near the cellular lines, in the bases of the latter, and in a particular tissue lying above the lower bed of the scale. This tissue has no particular organization; it is an amorphous tissue, like that in which the corpuscles of bone are deposited.

This tissue, which contains the corpuscles along with the cellular lines and their base, we shall call the upper layer of the scale. The organization which we have explained appears to us to bring it near to corpuscular cartilages, which are not ossified.

The fibrous layer.—If we rub off the upper surface of a scale with a knife, in so doing we remove the cellular lines, their bases, and the corpuscles, and we then have a view of the under layer, composed of fibrous lamellæ, the fibres crossing each other at regular angles, but all following the same direction in the same lamella. (Pl. III. fig. 14.)

If we tear the scale instead of scraping it, we then tear the under layer along with the rest, and thus divide the lamellæ into a great number of fibrous bundles, or even single fibres. This organization is altogether similar to that of fibrous cartilages.

When tearing a scale in this manner, it sometimes happens that we meet with fragments in which the cellular lines with their bases are seen to pass beyond the edge, an evident proof of the difference of these separate layers.

There are many scales in which we can perceive the inferior layer without the necessity of tearing it; such are the scales of the Carp. We then see at the side of the bases of the cellular lines very closely placed striæ, which are just the fibres of the under layer, and which are disposed in different directions.

This layer is thickest at the focus of the scale and thinnest at the edges. We shall afterwards see how the formation of the scales causes these differences in thickness.

It has been already stated, in treating of the longitudinal canals, that the latter are sometimes simple furrows formed by the interruption of the upper layer of the scale, and by the pressure alone of the under layer. If we examine the edge of scales (pl. III. fig. 4) we see distinctly how it is that the bottom of these canals is continuous with a broad space which we call the marginal space, and which is formed solely by the under layer of the scale. This space appears on the basal margin as well as on the lateral ones. If the fibres do not appear at first sight in these marginal spaces, they can always be rendered visible by tearing the scale.

These marginal spaces are represented not only in the scales indicated, where their formation by the inferior layer is most evident, but they are likewise seen in a great number of other scales.

The Focus. This is the name which we have applied to the point towards which all the longitudinal lines are directed, but which is not always situate in the centre of the scale. It is occupied by very large corpuscles, pale and thin, and by interrupted cellular lines, broken, as it

were, into small fragments. Such, at least, is the condition of the focus in the greater part of the hard scales of the Acanthopterygians; in the Malacopterygians, on the contrary, and especially in membranous scales, it often presents a circumscribed uniform surface, without corpuscles, and without interrupted circular lines. In such cases it is most frequently surrounded with concentric cellular lines.

The size of the focus varies; sometimes it occupies only a very small space, at others it is very extensive; it determines the dimensions of the different fields, because the lines of demarcation pass through a centre. Its form is sometimes round, and sometimes oblong, elliptical, square, &c.

We term that a granular focus which is occupied by corpuscles, numerous interrupted cellular lines, and cells more or less distinct, filled and varying in shape; that, on the contrary, we call a smooth focus, which has neither interrupted cellular lines, filled cells, nor corpuscles, on its surface.

The greater part of granular foci are situate beyond the centre, so as to render the basal field the largest, and the terminal one smallest; the majority of smooth foci, on the contrary, are placed in the centre, or very little beyond it; the fields are, therefore, more equal, and the form of the scale is in general oblong, with concentric cellular lines round the focus.

Teeth of Scales. The asperities found on the terminal edge of scales have not, till now, been subjected to a detailed examination. We must study these appendages in detail, in order to enable us to understand the various changes which they undergo.

The terminal edge of a scale of *Corvina nigra* is occupied by bodies whose form is represented at fig. 15, which affords a view of only a part of the terminal field. At the first glance we can perceive that their bodies are surrounded by an envelope. It was necessary, at the outset, to satisfy ourselves whether this envelope, and the body in its interior, were really separated from each other. This we ascertained by carefully tearing the parts asunder by means of a fine needle. In so doing these bodies themselves are sometimes torn, at other times the envelopes only are injured by being more or less deeply lacerated. We can thus remove almost all these bodies, and the membrane in which they are placed remains by itself.

Examined by themselves, we soon perceive in these bodies an organization similar to that observed in teeth; we first see a germ surrounded by a sac; this germ begins gradually to develope itself, acquires roots, and we can distinguish different layers in it; the extremity is unequal, truncated, or pointed. We conceive, therefore, that we may with propriety call these bodies the teeth of scales, even with a reference to the mode of their formation.

These teeth are least developed near the focus, and most highly in the first series, that is to say, in the series nearest the terminal edge. We have an example in fig. 15 of the two upper teeth in their sacs; the roots are very distinct; we perceive the thickness of the base of the tooth, and

there is no break throughout its whole length. In the two teeth immediately below these, the development is much less advanced, the ends are truncated; the external layer of the tooth does not entirely envelope it, but the roots can be distinctly observed, and we can even notice the roots of the different layers. Figure 15 likewise shews the different degrees of development, until we arrive at teeth of the most elementary kind, covered with corpuscles, ill defined, rounded, and very little developed.

We shall now describe a few forms in the different families of fishes.

The *Gobiöides* have a simple series of truncated teeth; the truncated surface is unequal, and of a deeper colour than the rest of the scale; the roots are sometimes very distinct, as well as the envelopes; their membrane is separated from the rest of the scale by a line.

The *Percöides* present well pointed teeth in the first series; the others are nearly square, and their sacs are visible at the sides; their position is alternate.

The *Solea nasuta* is provided with pointed teeth in all the series. It is very difficult to understand their position on these scales, on examining them for the first time, because the roots and lower portion of the sac of the upper tooth are covered by the end of the tooth and the sac of the inferior one. It is necessary to pay great attention in making the first observation.

In *Sargus annularis*, the teeth of only two or three of the first series are very distinct; the others are rudimentary and inconspicuous.

In the numerous series of teeth to be found in *Mugil cephalus* we have an opportunity of studying their successive development in detail. In the series next the focus, round sacs exist having a rounded germ in the interior; beyond that the sac is elongated into a point, the base of the teeth appears, the roots are developed, the pointed termination begins to form, and the entire tooth is at last confounded with the sac. The form of these teeth is ovoid, their position alternate. The teeth found nearest the focus, or rather what may be called the germs of teeth, are not very distinct, for they are covered with corpuscles and broken cellular lines.

Teeth are found on a portion of the scale which, among many fishes, such as *Tringla*, is easily destroyed, by being steeped in spirits of wine; this part then falls off along with the teeth, and it is always found separated at the place where the line occurs, which divides the teeth from the rest of the scale. The same fact is observed among the Gobiöides, &c.

The teeth are always most numerous in the middle of the terminal field, and they diminish in proportion as we approach the lateral edges, until at last we arrive at an insulated tooth.

Formation of the Scale.—If we would apply the results obtained by studying the intimate structure of scales, to the explanation of the manner in which they are formed, we will at once perceive, that it is of importance to distinguish between the formation of the superior layer and that of the inferior. The former composed of cells, and their bases with the tissue containing the corpuscles, acquires its development by the

augmentations which take place in the periphery, around the cellular lines; by means of such augmentations, they form not only many cellular lines, but these longitudinal canals become themselves elongated. I is very probable, that the cellular lines are formed not only one after another, but that many lines are produced simultaneously. We find a proof of this in the scales which preserve the marginal spaces during their successive augmentations, and whose cellular lines, or the cells, are thus separated into many groups. Of this the scales of *Cobitis fossilis* afford an example.

But this increase in the periphery will not explain the great thickness of the centre; we will find the cause of this in the formation of the inferior layer. We have seen that the latter is composed of numerous lamellæ; at each augmentation new lamellæ are always formed. The longitudinal canals which traverse the whole scale, convey the juices necessary to produce a uniform formation of a new lamella throughout the whole extent of the scale; it follows, that the old lamellæ, being smallest, the thickness must augment in proportion as we approach the focus.

The development of the teeth is likewise in accordance with this opinion. We have seen, in fact, that they are less perfect near the focus, that is to say, at the place where the longitudinal canals terminate; and that they are more developed the nearer they are to the margin. Development cannot take place without nutrition. That must, therefore, be progressive, from the focus to the margin: that is to say, the less developed teeth on the young scale acquire development on advancing towards the terminal edge. At the same time, new germs are forming behind them near the focus. It would not be difficult to recognise here a connection between this development and the longitudinal canals found in the part of the scale which is in relation with the skin.

The basal field is, in fact, almost entirely surrounded by the skin; we believe that we have even seen, on many occasions, the filaments which enter into the longitudinal canals. We see, then, after what has been said, what an intimate relation these canals have to the development, and that they perform, so to speak, the function of roots to the scale.

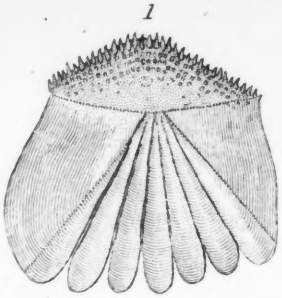
Perhaps an observation now to be noticed, may be regarded as still further corroborating this opinion. The scales of many species of the genus *Abramis* have appeared to us to lose their longitudinal canals at an advanced age; for we have seen some of them on other individuals which presented no difference but in size. These canals would then become filled, and, as it were, obliterated by age, a phenomenon by no means singular in anatomy.

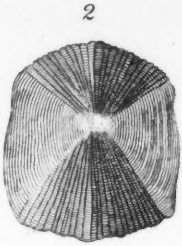
Do these canals, it may be asked, themselves convey the juices, or are they only tubes for the lymphatic vessels? Future researches must decide this question. For the present, we consider it enough to have demonstrated the relations which subsist between the growth of the scale and these canals, and the important part which the latter perform in the anatomy and physiology of the scale.

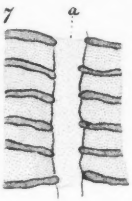
After what we have advanced, there is no need of repeating how much our opinion is opposed to that of authors, who consider the cellular lines to be the edges of the secreted layers of the scale.

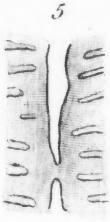
Our investigations have, therefore, proved, that scales are not to be considered as produced by the secretion of the skin, but that an internal nutrition, a true growth, takes place in these tegumentary appendages. Of this fact the teeth, found in the greater number of scales, furnish the most striking illustration, by the successive developments which they undergo: the canals, in their relation to the skin, tend to support this opinion; the cells (which may be termed granules when they are filled) undergo successive changes, as has been pointed out, and consequently prove that they are subject to augmentation and nutrition, and do not owe their origin to a secretion.

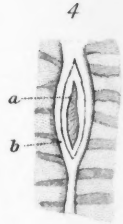
It is farther proved, that it is necessary to make a distinction between the two entirely different layers entering into the structure of the scale; layers different in their organization, their mode of growth, and the number of parts composing them; the superior layer, resembling corpuscular cartilages in its structure, presenting corpuscles and lines, differs essentially from the inferior fibrous layer. But here one point yet remains for ulterior researches to determine. We have stated that the fibrous lamellæ of the under layer, formed one after the other, gradually advance beyond each other, and that on the projecting edge of the most recent lamellæ, new cellular lines are formed, which thus constitute the continuation of the superior layer. Are these cellular lines formed altogether independently of the inferior layer, or are they the result of the elevation of the latter, which, however, by such an elevation, would undergo a complete transformation? This latter opinion is contradicted by the essentially different structure of the two layers; by the number of the inferior lamellæ being sometimes much more considerable, at other times, much smaller than that of the lines; and by the formation of the teeth, which is altogether independent of the inferior layer. But we might adduce, in support of this opinion, the transformation, for example, of cartilages into osseous tissue, two organizations essentially different; it may be alleged that a lamella produces numerous cellular lines on its surface,

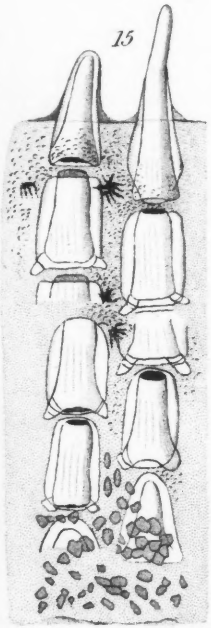


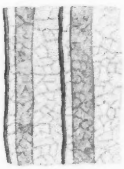


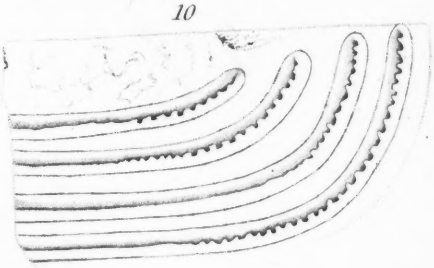


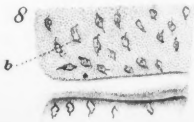


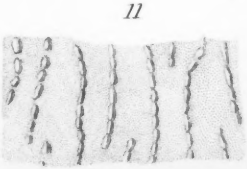


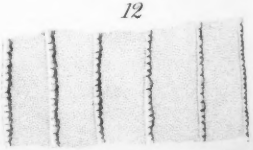












Structure of the Scales of Fishes, by L. Mandl.

XUM

or that all the lamellæ do not afford instances of this transformation. But, in either case, we cannot affirm that the edge of the inferior lamella constitutes these lines. We have in the first instance, seen the difference in the structure of the edge of the scale and the lines: the direction of the latter is sometimes entirely opposite to that of the edge; at other times, the lines situated between two canals are interrupted, and cross each other; while the corresponding edge of the scale follows a uniform direction, &c.

With regard to the other opinion, in favour of a formation from the upper layer, independently of the under one, we yet know too little of the matter to venture on an affirmation to that effect. Perhaps the study of the development of scales in young individuals, may throw light on a question full of interest in regard to the physiology of the tegumentary appendages.

The silvery matter deposited on the under side of the scale, on a particular membrane, consists of crystals, whose form is represented at Fig. 16.

EXPLANATION OF PLATE III.

Fig.
1. Scale of the Perch.
2. ... Carp.
3. ... *Cobitis fossilis.*
4. Portion of a scale of *Acerina vulgaris*, shewing the longitudinal canals.
5. ... *Mullus barbatus.*
6. ... *Serranus Scriba.*
7. ... *Percarina Demidoffii.*
8. ... *Scincus.*
9. ... *Corvina nigra.*
10. ... *Motella tricirrata.*
11. ... *Mullus barbatus.*
12. ... *Serranus Scriba.*
13. Portion of a scale of the same, shewing the corpuscles.
14. Inferior layer of a scale.
15. Teeth of a scale of *Corvina nigra.*
16. Silvery matter.

Remarks occasioned by Dr Mandl's Observations on the Structure of the Scales of Fishes. By M. Agassiz.*

I was informed at the time that M. Mandl had read to the Academy, on the 24th June 1839, the results of the observa-

* In a letter addressed to the Academy of Sciences, Comptes Rendu 3 Fèvrier, p. 191.

tions made by him on the structure of the scales of fishes, and that his conclusions differed from those which I had published on the same subject. Not being, however, precisely aware of the nature of his objections, I had necessarily to delay making a reply till his memoir was published. Since it appeared in the *Annales des Sciences Naturelles*, in October last,* I have carefully revised all the observations I published about ten years ago, on the *genera* and *species* of fishes brought from Brazil by Spix, and which I had described during my residence at Munich in 1829. I have also repeated my observations on the scales of fishes in general, the results of which I had stated in one of the introductory chapters of my *Researches on Fossil Fishes*. By mentioning these facts, I wish to convince the Academy that it is not by trusting to old recollections that I engage to refute the very trivial assertions of M. Mandl, but after a fresh examination of the whole question. It is of importance to me in other respects that I should insist on this point, and make the case known as it really stands, for M. Mandl so often repeats in his memoir, that the differences between his observations and my own, result from the inadequate means of observation at my disposal, that it might be supposed he was quite familiar with every thing that goes on in my study, instead of being, as he is, an entire stranger to me. M. Mandl, however, who piques himself on being so perfectly at home in every thing relating to microscopical investigation, both in France and in other countries, ought to be made aware that I possess an excellent microscope by Frauenhofer, of a peculiar construction, of which a description with plates, was published in 1830 by M. Dollinger, the illustrious physiologist of Munich, who has thrown so much light, by his microscopical researches, on embryology and the circulation of the blood in the capillary vessels. The instrument in question was regarded by this individual as the best then known.

I shall state further, that the observations, the result of which I am about to announce, were made on the scales of upwards of 300 species, belonging to all the families of the class of fishes, without taking into account the numerous observa-

* Mandl's observations will be found at pp. 113 to 126, and pp. 274—287. of this volume of the Phil. Journal.

tions made by me on the structure of the scales of fossil fishes. I shall now, however, limit myself to the statement of the mere general results of this inquiry, the details of which, accompanied with numerous figures, I propose to give in a memoir soon to be published.

M. Mandl alleges that I am mistaken in affirming that *scales are composed of superimposed plates.* He assures us, on the contrary, that they are formed of *cells placed near each other.* He even attempts to demonstrate this in the scales of the roach; and yet, in this very fish, I have succeeded in separating the plates of growth from each other, while, in numerous transverse sections of different scales, I have seen, with a magnifying power of 250 times the diameter, the superposition of these plates throughout the whole thickness of the scales. I have even already published a figure of such a section of the scale of *Salmo Trutta*, in my *Natural History of Fresh-water Fishes.*

M. Mandl afterwards affirms, that the divergent rays on the surface of scales which I had described as *furrows*, are true *canals.* I dare scarcely allow myself to believe, that M. Mandl has confounded the median tubes of the scales of the lateral line (which sometimes ramify to their posterior extremity) with the furrows of their surface; this would be to impute to him too gross an error; and yet I can perceive no other explanation of what he states. This much, however, I can positively affirm, that the other scales never have canals on their surface, but rather furrows interrupted above, and which are prolonged from the edge of a superior plate of growth, to the edge of the inferior one following, as is evidently shewn by every transverse section which can be made on any scale whatever which presents such rays.

M. Mandl further alleges, that the teeth of the posterior edge of pectinated scales are not *notches of the edges of their lamellæ*, but *true teeth having a root enveloped in a sac.* It is sufficient to examine the scales of the *Scienes*, which M. Mandl brings forward as an example, withdrawing and approaching the scale successively to the focus of the microscope, to be convinced, that all this pretended dental apparatus is nothing more than an optical illusion, resulting from the difference of the

thickness in these teeth at their base and apex; and that, in reality, the points which beset the posterior edge of the scales of the fishes, which I have named *Ctenöides*, are simply the more or less deeply marked notches of the edge, and not detached teeth.

Finally, M. Mandl seems to be completely ignorant, that enamelled scales exist, which very sensibly differ in their structure from those of ordinary fishes, and which are to be found in an order having the greater part of the species extinct, which I have named *Garöides*. Neither has he paid much attention to the examination of the shagreen of the Chondropterygians, which form my order of *Placöides*.

I shall enter into no farther details on the structure of the scales of fishes, *but merely conclude from my recent observations, that the description I formerly gave of them is correct, and that M. Mandl's mode of regarding them is in every respect erroneous.*

With regard to the application I have made of the study of scales to the classification of fishes, M. Mandl rightly blames me for one error. When I described the Mullets as *Cyclöides*, I possessed nothing but the scales of a single individual from Brazil, very ill-preserved, the small teeth, at their hinder edge, being completely removed by friction; but a new examination of many species of this genus has convinced me, that the Mullets are true *Ctenöides*, as M. Mandl has pointed out.

M. Mandl terminates his memoir by affirming, that I have united in the same family, fishes possessing scales of very different structure. To answer this assertion, I shall simply refer to the example cited by M. Mandl, viz. *Cobitis*, which I place, in common with all modern ichthyologists, in the family of the Cyprini. One may easily be convinced, by comparing the scales of Cobitis with those of *Gobio*, *Barbus*, and even with those of the Carps properly so called (provided we observe the young scales before their surface has been altered) that they present exactly the same structure: that is to say, they are formed of plates with simple edges, as among all my *Cyclöides*, and that, consequently, the charge of inaccuracy made against me falls to the ground.

I would have addressed these refutations to the Academy earlier, had I not been desirous, before doing so, of completely

revising my former observations, in order to preclude any farther dispute on the subject. The figures accompanying my memoir, which are all executed by Dr Vogt, who has assisted me in this protracted labour, will not, I hope, leave any farther room for doubt respecting the true structure of the scales of fishes.

On the best Means of Burning Gas for supplying Heat. By Sir John Robison, K.H., Sec. R.S.E., M.S.A.*

"*Vix ea nostra voco.*"

When carburetted hydrogen gas is employed in producing heat, it is seldom required that it should, at the same time, give out light; the combustion may, therefore, be managed in any way which may be convenient without seeking to preserve the illuminating power. It appears to have occurred about the same period to the late Dr Duncan and to myself, that, by passing a current of gas, mixed with atmospheric air, through a wide vertical tube, having its upper end covered by a diaphragm of wire-gauze, and by kindling the mixture as it escaped through the interstices of the wire cloth, a convenient stove might be formed for culinary purposes. Dr Duncan applied some small apparatus on this principle to pharmaceutical operations in his class-room, and I had my kitchen furnished with a range of large stoves, which were intended to supersede the use of French charcoal-stoves in various culinary processes. In both cases the success has been perfect, and the same principle has since been adopted with advantage in a variety of processes in the useful arts, where this neat and cleanly method of applying heat has rendered it a valuable acquisition to the work-shop. The form of the apparatus may be varied in any way to suit the particular process to which it is to be applied; as all that is essential is, that a current of the mixed gas and air shall rise through wire-cloth, and that the proportion

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* Read before the Society of Arts for Scotland, 13th March 1839. The special thanks of the Society awarded 18th December 1839.

of gas to atmospheric air shall never be so great as to allow of the flame becoming yellow, as, with this precaution, the combustion of the carburetted hydrogen will be complete, and no deposit of soot will take place on cold bodies when set over the flames; the proper quantity of gas in the mixture is easily determined by the stop-cock belonging to each stove.

For ordinary culinary purposes, the cylinders may be thirty inches long, and three to four inches diameter, and the wire-cloth for the tops should have about thirty wires to the inch. That which is manufactured for safety-lamps answers well for this purpose.

Whenever, from accidental injury or decay, a hole takes place in a diaphragm, it is no longer possible to use it; as, when lighted, the flame passes through the fracture, and communicates with the jet at the bottom of the cylinder, which then burns like an ordinary gas-light, and, like it, would blacken the surface of any cold body presented to it. The wire-cloths, if not broken through by violence, will last for months although in daily use; and, if covered by a layer of coarse sand, or pounded limestone, will continue serviceable for an unlimited period.

When more intense heat is required than is attainable by the unaided combustion of the mixed gases, recourse may be had to various forms of blow-pipes; and when a large volume of such flame is to be employed, the current of atmospheric air may be urged by double bellows. A very efficient apparatus on this principle is to be seen in the laboratory of Dr D. B. Reid.

It is to be regretted, that such applications of gas are not more generally known and introduced into work-shops, as there are numerous processes in the arts in which they would afford facilities to the workman which he can scarcely command by any other means. For example, in the hardening of steel-tools, it is well known that a piece of bright steel, when heated to redness in a forge or muffle, is subject to oxydation, and that a black scale remains after hardening, which it is difficult to remove without some injury to the work, as in the case of a screw tap; whereas, if the same piece of steel be

heated in a flame of the mixed gases, where there is no free oxygen to attack its surface, it may be *made and kept* red hot without any injury to its finest edge; it will be discoloured, but without losing much of its polish. The artist has also the advantage of a distinct view of the article while it is being heated, and the power of withdrawing it from the flame the moment it has acquired the proper colour which, in the hardening of cast-steel cutting tools, is of great importance.

Many attempts have been made to apply carburetted hydrogen and pure hydrogen gases to the purposes of warming buildings, and various forms of stoves have been proposed, on the understanding, it would appear, that, by applying the flame of the gas to metallic bodies, an increased degree of heat would be communicated by them to the atmosphere around. A little consideration will shew, that however the *distribution* of heat may be modified by such contrivances, there can be no increase of the heating power; and that when a certain measure of gas is fairly burned, the heat evolved into the apartment will be the same whether the flame be disposed as a light, or made to play against metallic plates or other combinations of apparatus. In all cases where the products of the combustion are allowed to mix with the atmosphere of the apartment, without provision being made for carrying them off by ventilation, the effects of such processes must be more or less deleterious to health, according to the proportion these products bear to the mass of air they mix in. On the whole, it may be assumed, that this mode of heating apartments is the most expensive, the least efficient, and, excepting that by Joyce's charcoal stove, the most insalubrious that can be resorted to.

On the best Method of Burning Gas for the purpose of Illumination. By Sir John Robison, K. H., Sec. R. S. E., M. S. A.*

The theoretical principles on which carburetted hydrogen gas may be used with the best advantage, for the purpose of

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* Read before the Society of Arts for Scotland, 27th March 1839. The special thanks of the Society awarded, 18th December 1839.

domestic illumination, have been so well laid down by the late Dr Turner, and by Dr Christison, as well as by other chemists, that it would be superfluous to enter at all on this part of the subject in a paper, the object of which is to give such practical directions for the proper construction and management of gas-fittings, as may lead workmen to give the requisite forms and proportions to the parts, and may enable the consumers to obtain the quantity of light they require, from the smallest practicable expenditure of gas, and with the least possible inconvenience from the products of its combustion.

It is very generally believed, by workmen and others, that the more freely the current of air is admitted to an argand burner, the better will be the light; and hence the burners and glass chimneys in ordinary use are made in such a way as to favour this view. No practice, however, can be more incorrect, or can lead to less economical results. An attentive observation of what takes place will shew, *that there is only a certain proportion of air required for the favourable combustion of a definite measure of gas.* If more air than this due proportion be allowed to pass up the chimney, the size of the flame will be reduced, and the quantity of light diminished: if, on the other hand, less than the due proportion be admitted, the surface of the flame will be increased by elongation, but it will become obscure, and the quantity of light will decrease, owing to the escape of particles of unconsumed carbon. A simple experiment will exemplify this. If the flame of an ordinary argand burner be reduced, by partially shutting the cock, to about half an inch high, the light will be pale and blue, because the supply of air is too great for the small quantity of gas which is issuing. If partial obstruction be given to the access of air, by applying a handkerchief under the burner and chimney, it will be found that the size of the flame and the quantity of light emitted will increase until it arrive at a maximum, when, by farther obstruction, the admission of air will be reduced below the proportion required for the burning of the carbon, and the light will diminish.

It appears, therefore, that the proportionate size and shape of the burners, and the diameter and height of the glass chim-

neys, are by no means indifferent matters, but that much advantage may be gained or lost by giving them such forms and proportions *as may insure the development of the maximum degree of light which the gas is capable of affording.*

As a general rule, it may be considered that in all burners, whether well or ill made, the greatest quantity of light, *in proportion to the gas expended*, will always be obtained when the flame has been raised as high as it will go without smoking. In proof of this, the following experiment may be made. In any situation where there are three or four burners of the same size, and with similar chimney-glasses, and receiving their gas through a meter (by which the expenditure may be measured), if one of these burners have its flame elevated as high as it can be made to burn without smoking, and if its expenditure per hour be accurately noted on the meter; if the other two or three burners be then lighted, and their flames be so regulated that their united lighting power shall be just equal to the large flame of the first burner, it will then be found, on noting the expenditure, that it is much greater than in the case of the equal light from the single burner, and that the first burner, which gives light equal to two others, consumes but two-thirds of the gas which they do, or, if it be compared with three others giving together an equal degree of light, its consumption will be little more than half of theirs. It follows from this, that when a certain degree of light is required, such a burner should be employed as *is capable of giving this light and no more*, and that it is bad economy to use a more powerful burner with a flame of less than its due height. This rule holds good with any number of burners, and is equally true whether they be well or ill made.

The same rule will apply to the individual jets of an argand burner, as holds in regard to their united effect, and if, in any burner, the jets be of unequal heights, in consequence of bad drilling of the apertures, or neglect of keeping them free of dirt, the consequence will be, that when the flame is raised until the jet from the widest hole reaches the most advantageous height, those from the obstructed holes will be consuming the gas at a disadvantage, which will be greater or less

according to circumstances, but will always be of greater amount than is generally supposed.

The experiments made by Drs Turner and Christison serve to shew, that much smaller chimneys than those usually employed, are required to burn the gas to the best advantage. Unfortunately, however, the dimensions most favourable to economy in one respect, are beyond the limits of economy in another; and when the glasses are made small enough in diameter to obtain the maximum of illuminating effect, they are liable to be softened by the heat; or to be cracked, *if not accurately centered.* A compromise between the two evils must therefore be made, and if this be judiciously done, *a great improvement on the usual routine practice may be effected, a more beautiful and steady light be obtained at a less cost, and our domestic comfort be increased, by the diminution of the heat and effluvia of the gas.*

For practical purposes, therefore, the following directions may be observed.

Whatever diameter is given to the burner, the glass chimney should not exceed it by more than half an inch at the utmost. If the burner be less than three-fourths of an inch in diameter, the chimney-glass should not exceed $1\frac{1}{8}$ inch in internal diameter. In any case, its height should be no more than four inches above the mouth of the burner from which the jets spring.

The smallness of the interval which is in this way allowed between the flame and the glass, renders it necessary that the workmanship of the supporting gallery be accurate, *in order that the chimney may be held perpendicular, and truly concentric with the flame.* Gas-fitters rarely give sufficient attention to this important point, and a large share of the expense from broken glasses is owing to defects in this particular.

In the ordinary mountings, the gallery is put on the burner, which it seldom fits accurately, the glass likewise rarely fits tight into the socket of the gallery, and from these two causes, it is often so much off the centre, or so far from being upright, that the flame cannot be raised to a proper height without risk of breaking it. This risk may be greatly diminished by a little change in the disposition of the burner and gallery. In-

stead of hanging the gallery on the burner, it should be placed beneath it, and fixed by screwing down the burner on it. In this case, it is necessary to give the gallery an increased diameter, as the air, both for the inside and the outside of the flame, must enter through its ribs. The burners should also be made conical instead of cylindrical; but this is not so important as drilling them with numerous holes,—at least double the number usually allowed, as the closer they are the better, the expenditure being regulated by the stopcock, and not by the number of holes.

In making the galleries, great attention should be paid to having the rim and seat for the glass truly concentric with the hole through which the nozzle-screw, on which the burner is fixed, passes; the workmen should have a solid wooden chuck of the size of the bells of the chimney-glasses, and should chuck the galleries on it, in order to drill the aperture through which the nozzle-screw is to pass. The outside and inside faces of this hole should at the same time be turned true, as, if this be done with the proper care, the glass, the burner, and the gallery, will all be true to the same axis, when they are put together and screwed up. The hole through the gallery should not be tapped, as the burner is sufficient fixture for it when screwed down over it. If this part of the work be well executed, even an indifferently made burner will perform well, and if it be ill done, the best burner will appear defective, and be liable to break the glasses.

The arrangement of burner and gallery here recommended is not incompatible with the use of plain cylindric glasses, but it will be found better to use what is sometimes called the French-shaped chimneys, that is, those which are used with the common argand oil-lamps. The wideness of their mouths gives them a firm seat in the gallery, and if the length of the bell, or wide portion of the glass, be such that the neck or choke shall be on the level of the lip of the burner, and the upper part of the glass be four inches to four inches and a half long, then a favourable result will be obtained. It is expedient to obscure the lower part or bell of the glass, as the burner is thereby concealed, and the flame appears to rise out of a thick wax-candle. No moon-shades should ever be used,

as, besides intercepting a considerable portion of the light, they prevent the consumers from observing whether the burners and glasses be in good order, and performing properly.

It is pretty generally imagined that the smoking of ceilings is occasioned by impurity in the gas, whereas in this case there is no connection between the deposition of soot and the quality of the gas. The evil arises either from the flame being raised so high that some of its forked points give out smoke, or more frequently from a careless mode of lighting. If, when lighting lamps, the stopcock be opened suddenly, and a burst of gas be permitted to escape *before* the match be applied to light it, then a strong puff follows the lighting of each burner, and a cloud of black smoke rises to the ceiling. This, in many houses and shops, is repeated daily, and the inevitable consequence is a blackened ceiling. In some well-regulated houses, the glasses are taken off and wiped every day, and before they are put on again, *the match is applied to the lip of the burner, and the stopcock cautiously opened, so that no more gas escapes than is sufficient to make a ring of blue flame*, the glasses being then put on quite straight, the stopcocks are gently turned, until the flames stand at three inches high. When this is done, few chimney-glasses will be broken, and the ceilings will not be blackened for years.

Gas-fitters and lamp-makers generally put the stopcocks in situations where it is difficult to get at them, and they make their heads, so small that, if they be in the least degree stiff, it is not easy to turn them gradually; hence, when a little force is applied, they move by starts, and the flame is sometimes raised too high, or, instead of being a little lowered, is altogether extinguished. The remedy for this inconvenience is to make the cocks easily accessible to a person standing on the floor, and to make their levers so long that their movements may be easily graduated. The cocks and levers may easily be designed so as to form part of the ornamental work of the lamps.

The argand burner being the most perfect and economical which can be used, unless where small portions of light are required, it is unnecessary to say any thing of the batwings and other fancy burners, especially as the only precau-

tion to be taken with them, is to take care not to raise them so high as to smoke, and never to use two or more low flames, when the same degree of light can be got from one flame burning at its most effective height.

A mode of supplying argand burners with a current of heated air has been lately proposed in Paris, and much praised in London. This is effected by having an outer glass of a diameter a little larger than the inner one. This outer glass reaches farther down than the bottom of the burner, and is closed below by a metal plate; the air for the supply of the flame is made to pass down between the outer and inner glasses, where it gets heated; it then enters the inner glass and the centre aperture of the burner, and passing upwards, supports the combustion of the gas in the usual way. There is no doubt that, by this arrangement, a considerable improvement may be made in cases where ill made burners, with wide and tall chimney-glasses are employed; but if the experiment be tried with burners and glasses proportioned in the way recommended above, it will be found that no advantage is gained, and that the maximum effect has been attained by a simpler apparatus.

Before quitting the subject of burners, it may be right to advert to a frequent cause of disappointment in their performance. The perfection of an argand burner is, *that the flame arising from it should appear in a continuous cylindric sheet, with a smooth upper edge, and without forking points.* This is sometimes very difficult of attainment, however carefully the jet-holes may be gauged by the pricker. There are two causes for this irregularity; one is, that, if the drill which is used be blunt, a little blaze is pushed aside by it when it is forced through the plate in which the jet-holes are pierced; this blaze adheres to the edges of the hole, and interferes with the passage of the gas, and being unequal in its effects, renders the flame forked. The other cause is, that the inside of the burner is seldom turned true, and that the shoulder in which the pierced disk rests, is not of equal width all round, and sometimes may be so thick in some places, that the drill, when it gets through the disk, strikes against the shoulder; this likewise interferes with the issue of the gas. To avoid these causes of irregularity, the following precautions are essential.

When the seat for the disk is turned out, the inside space between the inner and outer walls of the burner should be turned true for a quarter of an inch inwards, and no more shoulder should be left than just enough to support the disk in its place. The disk should then be put into its seat, but not finally fixed. The requisite number of holes should then be drilled in it, and slightly counter-sunk to take off the barb. The disk should then be reversed (that is to say, the counter-sunk face should be put inwards), and finally fixed in its place. The blaze which may have been pushed through with the drill will now be on the outside, and may be easily removed by the file, or by a slight counter-sinking, which is the preferable manner, as the smooth-edged holes will keep longer clean than those with a sharp arras, the application of an old tooth-brush being sufficient to keep them in good order.

The above observations apply chiefly to the illumination of the interiors of buildings, and it may be proper to notice the circumstances which require to be attended to in lights which, being placed externally, are in some degree exposed to the weather. The most important of these are the street lamps. These may either be arranged at considerable distances, and be fitted with powerful burners, or the intervals between them may be smaller, and only a single jet be allowed for each. Various local considerations must determine this, as well as some other points; but it should be kept in mind, that the best small light is either the single jet of three inches and a half or four inches high, or the fish-tail jet of three inches high, and that for more powerful lights *the argand is preferable to all others.* The large bat-wing, so much used in large public lamps, is wasteful, smokes the lantern, and does not give light in proportion to its expenditure.

In most towns, framed square lanterns are used for street lamps, as it is said that globes are apt to be obscured in cold weather by the deposition of the water generated by the combustion of the gas. It is no doubt true, that if proper precautions be not taken, this inconvenience would be felt, and the water which would trickle from the aperture in the bottom of the globe would be liable to freeze in severe weather, and so to close up the access for air, that the lamp would smoke or

go out. In all other respects, globes have undeniable advantages over framed lanterns, as they protect the flames better in high winds, and they are kept up at much less expense.

Some years ago I pointed out to the Commissioners of Police of this city, that advantage might be taken of the acknowledged good properties of the globes for lighting the streets, and the alleged defect be obviated by constructing the tin tops with chimneys which should reach down to the points of the flames, and by their current carry off the water while still in a state of vapour, and so prevent it from being condensed on the sides of the globes. This plan was immediately tried, and having been found successful, was adopted in all the lamps erected subsequently. It occasionally happens, that from the jet being deranged, the gas is directed past the tin-chimney instead of into it, and if the weather be cold, it is immediately observed that a deposition of water takes place inside the globe, and its sides become dim. The adjustment of the burner restores the proper action, and the globe remains bright. This plan having now stood the test of many years' experience may safely be recommended for adoption wherever new street lamps are erected.

Abridgment of an Essay on the Construction of Oblique Arches.

By Edward Sang, M.S.A., Civil Engineer, Edinburgh.
Communicated by the Society of Useful Arts for Scotland.*

Scarcely any branch of civil engineering bears so closely on the advancement of civilization as the art of road-making. The immense sums that are annually expended on them evince the importance of our roads. Our object is not merely to find a path from one town to another, we must be transported in the most expeditious manner possible. Is there a declivity? thousands are spent to remove it: Is a road suspected of being a few yards longer than is needed? a new line is imme-

* Read before the Society for the Encouragement of the Useful Arts in Scotland, on 18th November and 2d December 1835; 27th January 1836, and 16th May 1838.

diately chalked out. One might almost imagine that a monomania had seized us, and that the tulip, the dog, the pigeon, and all the other fanciers had deserted their peculiar departments to concentrate their energies on this one grand matter of roads. The madness is a very reasonable one; for if there be a hill, multitudes daily climb and descend it; or if a road be circuitous, the quantity of unnecessary travelling might soon be sufficient to carry one comfortably round the globe.

While journeying, we are often annoyed by bridges. Sometimes, for cheapness, they have been erected far out of the line of road, and we enjoy on one side of a river the delightful prospect of doubling along the other. At other times, after skirting the banks as if on a journey to the source, we are all at once wheeled right across the water, and ere we are certain that our necks are yet safe, an equally sudden turn restores us to our original direction. And occasionally our vexation is crowned by an altercation between the drivers as to which of two vehicles is bound to back down the steep slope of some antiquated erection. That time has now gone bye when a bridge of any kind was hailed with satisfaction; we have scarcely such a thing as a ford wherewith to contrast it, and having only bridges to compare with bridges, we have become somewhat nice in our taste. Many of the old high-backed bridges have been replaced by others with level road-ways; these again by bridges with road-ways inclined to suit the elevation of the opposite banks, and now another improvement is beginning to be called for, that of crossing the river obliquely, so as to make the bridge harmonize with the general line. This we may consider as the *ne plus ultra* in bridge building, for then the road-way over the bridge coincides both in plan and in section with the rest of the road, and therefore conducts us in the easiest manner possible from the one bank to the other. The skewed arch is inseparable from the railway, as by its introduction alone the engineer is able to free the line from awkward and injurious turnings.

Having been consulted concerning the construction of an oblique bridge of considerable magnitude, and never having met with any regular investigation into the theory of such structures, I was induced to undertake the analysis. The

results of that analysis I proceed to lay before the Society of Arts, in the hope that, though I may be wrong in supposing them new, their publication may serve to disseminate correct notions on this intricate subject. It is a common idea that the oblique is weaker than the right arch, and that the twist of the stones causes a great waste of material. The truth is, that if both bridges be skilfully constructed, there is no difference in point of strength between them, while the twist on the arch-stone of the oblique bridge causes a most trifling loss of matter, and therefore our road trustees should never hesitate to adopt that which agrees best with the rest of the line. There is no limit to the obliquity, nor need even the several abutments run parallel with each other.

The general question of the construction of an arch resolves itself into two parts; the first relating to the connection which ought to exist between the curvature of the vault and the weight piled on each portion of it, is absolutely identical in the two cases of right and oblique bridges, and is therefore left out in the present inquiry; the second, however, relating to the forms of the arch-stones, bears directly on the oblique arch, and will therefore engross almost our whole attention. The outline of the bridge and the form of the vault having been determined on, the problem becomes this; *To cover the surface of the centering with blocks of such sizes and forms as may insure the stability of the structure.* Now, if it be premised that the curved surface of the vault must never be vertical, the solution of the problem can always be attained.

It is clear, from the general form of a bridge, that the lines of pressure ought to run from one abutment to the other, and should be contained in vertical planes parallel to the walls of the parapet. Imagine, then, that the vault is intersected by a multitude of such planes, the lines of intersection will indicate the directions in which the pressures ought to be transmitted from block to block. Now the stability of a structure is obtained by making the surfaces at which the pressures are communicated perpendicular to the directions of those pressures, and therefore all that is required is to trace on the surface of the centering a line which may cross all the lines of pressure at

right angles. In the case of the right arch, that line is a parallel to the abutment; but in the oblique arch it becomes bent in a peculiar manner.

At the crown of the cylindrical oblique arch the joint-line is perpendicular to the parapet; of course, it begins to descend on the surface of the vault, and as it descends it gradually bends away from that direction to become more and more nearly parallel to the abutment. If the crown line be regarded as the abscis, and the line of pressure as the corresponding ordinate of the joint, the differential co-efficient of the line of pressure is in all cases proportional to the cosine of the inclination which its extremity has to the horizon. If there be, then, two closely contiguous joints, the portions of the lines of pressure intercepted between them will be proportional to the cosines of the obliquities, and hence it follows that the breadths (measured on a line of pressure) of the stones in a given course diminish in the ratio just mentioned. It is a well known principle, that the strain upon any arch stone is proportional to the secant of the same obliquity; and thus, if the depths of the stones be augmented to meet this increased strain, it would follow that each voussoir in any given course ought to exhibit the same extent of section by a plane parallel to the parapet. The arch stones, both for convenience of workmanship and for appearance, must be uniformly disposed from side to side; and hence throughout the whole structure they ought to be of uniform volume, with the exception of the half stones left at the end of each alternate course for the purpose of *breaking the joint.* The deepening of the arch-stones toward the spring of the arch is often, though very improperly, omitted; in such case the above statement does not hold true.

Even although the arch-stones were all equally broad upon the centering, those nearer the abutments would appear narrower on the Ground Plan, the breadths of their projections being proportional to the cosines of their obliquity: Hence the ground plan of an oblique arch must present a very rapid diminution of breadths toward the spring of the arch, the breadths of the projections being, indeed, proportional to the squares of the cosines of the obliquities.

The Side Elevation of a vault with uniform voussoirs would exhibit narrower intervals toward the crown, the breadths being proportional to the sines of the obliquities; hence the side elevation of a skewed arch must present narrow intervals both at the crown and at the abutment, and wider intervals upon the shoulders. The breadths are proportional to the products of the sines by the cosines of the obliquities; that is, to the sines of twice the obliquities; and thus the side elevations of those arch-stones which are inclined at 45° will be the broadest.

The End Elevation, or the projection of a joint upon the plane of the parapet, possesses the very singular property of being entirely independent of the angle of the skew, and of depending alone on the form of the longitudinal section of the vault. This curious fact can very readily be demonstrated. The projection of a right angle upon a plane parallel to one of its sides is always a right angle, and therefore the projection of the joint upon the plane of the parapet must cross the projection of every line of pressure upon the same plane perpendicularly. But the projections of all the lines of pressure are equal to, and placed side by side with, each other, and are so whatever may be the angle of the skew, so that the delineation of the end elevation of a joint, which requires only the tracing of a line that may cross all these at right angles, will be performed exactly in the same manner whether the bridge be more or less oblique. When the angle of obliquity diminishes to zero, that is, when the bridge becomes *right*, the end projections of the joints contract into mere points, which points are the commencements, so to speak, of the permanent curves above mentioned.

The end elevations of the *beds* of the voussoirs, or rather of the lines formed by the intersection of these beds with the planes containing the lines of pressure, are also normals to the lines of pressure, and must therefore be tangents to the end projections of the joints. From this it follows, that a short portion of a course, or a single arch-stone, is very nearly contained between two planes slightly inclined to each other; and that, therefore, the loss of material arising from the *twist*

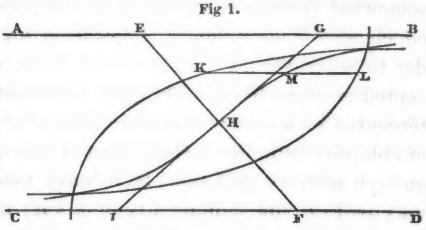
of the stone must always be insignificant. Those engineers who have experienced a loss on this account, have done so because their bridges were not properly designed. If the stones be obtained in *squared blocks* from the quarry, there will be a loss on the ends of the stones; but this, as every builder knows, can be avoided by proper management in the quarry. And thus, on the whole, the loss of material for the skewed bridge need not exceed to any extent worth naming that for the right one.

The above statements are true of cylindroid oblique arches *whatever may be the forms of their principal sections;* they are at variance with the statements and so-called experience of engineers of established reputation: complete demonstrations of them are given in the appendix. They are equivalent to differential equations, and require to be integrated in order to give practical results; these results vary according to the particular form assumed for the longitudinal section of the vault. I proceed to give a few of these results, commencing, on account of its more frequent occurrence, with the circular arch.

On investigating the form of the projection of a joint of a circular oblique arch upon a horizontal plane, I arrived at a new curve, to which the name *Double Logarithmic* has been given.

Having projected the entire semicylinder, of which only a portion can be used with propriety, let AB, CD, be the sides of the projection, and EF, parallel to the parapet, the plan of one of the lines of pressure. Bisect EF at right angles by GHI, and form two logarithmic curves of which AB, CD, may he the asymptotes, EG the common subtangent, their ordinates being parallel to EF. Then draw lines KL parallel to AB, and intercepted between the logarithmics, the middles M of these lines trace out the horizontal projection of one of

Fig 1.



the joints. The lines AB, CD, are thus asymptotes to the horizontal projection, and this geometrical property illustrates the mechanical impossibility of constructing a semicylindric arch, without trusting to the cohesion of the mortar. The introduction of the logarithmic curve into investigations concerning bridges, has been of great utility, and the analogy between this curve and the common catenary is striking. The catenary is also formed by bisecting the interval between two logarithmics; but these have a common asymptote with rectangular co-ordinates, while the bisected line is parallel to the ordinate. The computations needed for the delineation of such projections, are by no means tedious: they may be performed rapidly by help of Napierian logarithms; but a better method, capable of giving all the projections, will be explained shortly.

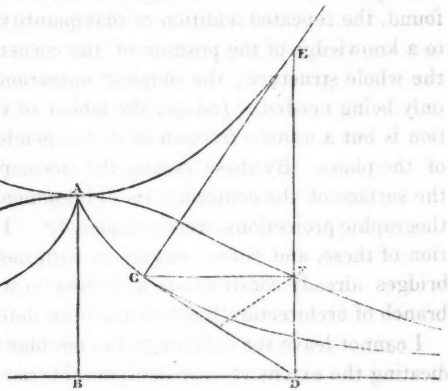
It may be expected, from what has been said of such elevations in general, that the end elevation of a circular oblique arch shall present some interesting peculiarity. The end elevation of a joint ought, in fact, to cross at right angles the circumferences of circles described with equal radius from points lying in a straight line; now, this is the distinguishing characteristic of the tractory, and that curve must therefore be exhibited on the end projections of all circular oblique arches.

On examining the projection of one of the joints upon a vertical plane perpendicular to the parapets, I obtained the genesis of a peculiar curve still logarithmic in its nature, and somewhat resembling in its form the superior branch of the conchoid. If we conceive the side elevation of the semicylinder to be traversed by horizontal lines, the distances intercepted on these lines bear to the corresponding distances intercepted by a certain normal curve, the ratio of cotangent of obliquity to radius. This normal curve, which belongs to an arch with its obliquity 45°, I have named the COMPANION TO THE TRACTORY; it admits of a very neat mechanical delineation.

Let a rod AB, equal in length to the radius of the arch, be made to rest upon a smooth board only at the point A, while

the extremity B is guided along the line BD; A will, as is well known, describe the equitangential curve or tractory. Suppose that the guide to which the point B (or in an oblique position D), is attached, carries a vertical rule DFE, and that, on that rule, there slides a right angle DFC, one side of which is constrained to pass through C :* then will the point F trace the *Companion to the Tractory*. A very simple addition will convert this instrument into that described by Leslie in his Geometry of Curve Lines, for forming the catenary. A grooved rule has only to be attached, making the right angle DCE, while the groove DF is continued to meet it: E then traces out the catenary. Since, *from the nature of the figure*, ED DF = AB^2, it follows, that the companion to the tractory has its ordinates inversely proportional to those of the catenary, and that, therefore, it might, with propriety, have been named the *inverted catenary*.

Fig. 2.



All these projections of the joints, and the forms too of the individual arch-stones, can be much more readily obtained from the delineation of the surface of the centering. Regarding the crown line as the abscis, and the actual lines of pressure as the ordinates (on the curve surface), half the ordinate plus 45°, has its logarithmic tangent proportional to the abscis.

* In practice, it would be more convenient to lay a jointed rod equal to half AB from the middle of AB to the rule DFE as indicated by the dotted lines.

Having once obtained the log-tangent corresponding to a given distance along the crown line, a simple proportion will give that corresponding to any other absciss; the log-tangent corresponding to half the length of an arch-stone having been found, the repeated addition of that quantity to itself will lead to a knowledge of the position of the corner of each stone in the whole structure; the simplest operations of trigonometry only being needed. Indeed, the labour of the whole calculation is but a minute fraction of that expended in the drawing of the plans. By these means, the accompanying model of the surface of the centering, its development, and various orthographic projections, were completed.* The simple inspection of these, and their comparison with most of the skewed bridges already constructed, will shew in what respects this branch of architecture has hitherto been defective.

I cannot leave the subject of the circular arch without indicating the extensive and indispensable use of logarithms in the calculations. Napier, when he founded first the rudiments of the fluxional calculus, and thence the logarithmic method, sanguine though he may have been as to the immense value of his discoveries, could never have imagined the prodigious impulse which they have since given to every branch of exact science. Each new mathematical research piles another stone on the monument of Napier.

Neither can I avoid remarking, that the ingenious speculations of the earlier geometers concerning the various mechanical curves, speculations which have been by many regarded as fanciful and useless, are one by one turning to account in the progress of modern philosophy.

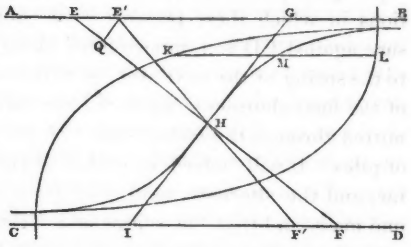
The elliptic arch, being much recommended by the gracefulness of its form, is frequently used. If we view the circular oblique arch from a distant point in the continuation of its axis, it does indeed appear elliptical; but then the ellipse has its major axis directed vertically, so that a circular skewed bridge can hardly have a fine appearance unless the segment be extremely flat. Let us then inquire into the phases of an elliptic skew.

* These are deposited in the Museum of the Society of Arts of Scotland.

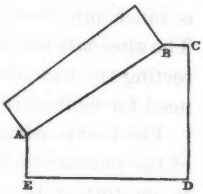
The horizontal plan of the joint is still a double logarithmic curve; and its delineation, including, of course, that for the circular arch, is as follows.

EF being, as before, the plan of one of the lines of pressure, find HQ a third proportional to the horizontal and the vertical semi-axis; through Q draw Q'E parallel to HG. Describe then logarithmics having E'G for their common subtangent, and having their ordinates parallel to E'H, the bisection of the interval between these will give the horizontal projection of the joint. Similarly, the side and end projections are modifications of those belonging to the circular arch:—they are fully investigated in the appendix.

Fig. 3.



Having obtained a tolerable approximation to the forms of the arch-stones, it is not uncommon for bridge-builders to throw the remaining responsibility on the abutments, which, besides transmitting the pressure, have to continue its distribution among the parts of the pier. In truth, the principles of equilibrium seem never, even in the case of the right arch, to have penetrated beyond the facing stones of the piers; and the effect of the arrangement *in every bridge which I have seen, or the drawings for which I have inspected*, is to throw the whole weight of the arch on the outside stones of the pier and on the outer row of piles in the foundation. To see this clearly, let us draw one of the abutment stones of a right bridge. The oblique face AB receives the pressure of the lowest voussoir; and it ought to receive that pressure perpendicularly. But the stone is prevented from yielding by resistances against the surfaces CD, DE: the pressure of the voussoir is thus decomposed into two pressures, one against CD, well known to



be the horizontal thrust of the bridge, and the other against DE, equal to the weight of all the mason-work between the crown of the arch and the vertical line through B. Now, since all the stones of the piers are squared, no change (except by improper straining) can take place in the directions in which these pressures are propagated. The pressure against CD is communicated along the abutment course to the spring of the next arch, or to the corresponding breadth of the final abutment; while the pressure against CD is transmitted through the facing stones of the pier to the outer row of piles. It will, indeed, be said, that the cohesion of the mortar, and the alternate jointing of the courses, render the pier one mass, and that, therefore, such niceties are not worthy of attention. But, indeed! is the final disposal of the entire strain of a bridge, such a trifle? Then let us fit our arch-stones by guess, and sweep the span in any fancy. It is at this very corner that all the care of the engineer is required; and I do maintain, that the method in common use outrages the doctrines of equilibrium, and renders our arches less secure than they ought to be. It is a piece of bad engineering to throw the whole weight of a bridge upon one row of its supports, and to give the others scarcely any strain; especially when it is considered that that row is most liable to decay. The alternate jointing of the stones calls into action that species of resistance which ordinary building-material is least capable of exhibiting: one end of a stone is pressed downwards, while its other end is engaged between two blocks; the consequence is a tendency to break the stone over, to *distend* its upper surface; and it is notorious that the strength of stones in this way is much inferior to their power of resisting a simple crush. The alternate jointing and the mortar are useful enough in correcting the bad effects of unavoidable inaccuracy: there is no need for deliberate error to put them to a severer use.

The best possible arrangement is to give to each square foot of the foundation its fair share of the whole burden. In order to do this, it becomes necessary to lay a counter arch, of a parabolic form (its convexity downwards), upon the pier-head. Such an abutment course would carry the horizontal thrust to the spring of the next arch, precisely as a flat course would;

but it would distribute a uniform downward pressure on each horizontal foot; and, in this way, the foundation would be pressed on exactly as if the whole weight of mason-work, from the crown of the one arch to the crown of the other, were piled upon it in squared courses.

On investigating the forms of the joint on a parabolic skew, I found its plan to be a line of the third order, the double parabola; that its end elevation is a semi-cubic parabola; and that its side elevation is another line of the same order. Students of the higher mathematics will at once recognise the equations of these curves as the results of other inquiries. For the computations of the parts, on account of the regular progression of the different examples, the method explained in my treatise *On the Solution of Equations of All Orders*, will be found to afford peculiar facilities.

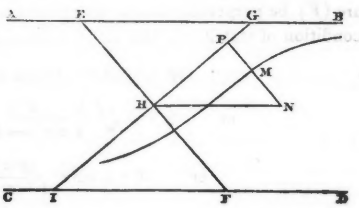
Appendix.

In the preceding part of this paper, I have stated the general principles which ought to regulate the construction of oblique arches. In this, the second part, I propose to enter more into detail, and to give the demonstrations of the theorems above laid down.

The general investigation into the stability of a vault would necessarily be complicated, by the peculiarities of the ultimate abutments, and by the assumed directions of the lines of pressure; for these directions are, within certain limits, arbitrary. For the present purpose, it is enough to consider the case of a vault resting on parallel abutments, cylindroid, and having the lines of pressure contained in vertical planes parallel to each other.

Let AB, CD, represent the two abutments, HN the crown line, EF and PN the horizontal projections of two of the lines of pressure.

Of rectangular co-ordinates, let the x be in the direction HG, the y in PM, and the z vertically.



For convenience, also assume oblique co-ordinates v along HN, u along NM, and z as before: put also GHN the angle of the skew, $= s$. The formulæ of conversion will be

$$\left.\begin{array}{l} x = v\cos s, \quad y = v\sin s - u; \quad z = z \\ v = x\sec s, \quad u = x\tan s - y, \quad z = z \end{array}\right\} \quad . \quad . \quad \text{A}$$

If the equation of the generating curve of the vault, of which EF is the projection, be taken

$$u - \varphi z = 0 = \text{B}$$

the same equation will serve as that of the vault itself; or in rectangular co-ordinates

$$x\tan s - y - \varphi z = 0 = \text{B, whence}$$

$$\left(\frac{d\,\text{B}}{d\,x}\right) = \tan s; \quad \left(\frac{d\,\text{B}}{d\,y}\right) = -1; \quad \left(\frac{d\,\text{B}}{d\,z}\right) = \varphi' z.$$

The equation of the plane containing one of the lines of pressure, is

$$x - \text{X} = 0 = c; \text{ whence}$$

$$\left(\frac{d\,c}{d\,x}\right) = 1, \quad \left(\frac{d\,c}{d\,y}\right) = 0; \quad \left(\frac{d\,c}{d\,z}\right) = 0;$$

so that the equations of the straight line touching B $=$, 0 $c = 0$ are

$$\frac{\text{X} - x}{0} = \frac{\text{Y} - y}{-\varphi' z} = \frac{\text{Z} - z}{-1} \quad . \quad . \quad \text{(D)}$$

where X, Y, Z belong to any point in the tangent; x, y, z to the point of contact.

Again, let $u - \theta v = 0 = \text{E}$ be the equation of the horizontal projection of a joint, or in rectangular co-ordinates,

$$x\tan s - y - \theta\,(x\sec s) = 0 = \text{E; then}$$

$$\left(\frac{d\,\text{E}}{d\,x}\right) = \tan s - \sec s.\,\theta' v; \quad \left(\frac{d\,\text{E}}{d\,y}\right) = -1; \quad \left(\frac{d\,\text{E}}{d\,z}\right) = 0.$$

The equations of the joint are B $= 0$, $c = 0$, therefore, those of a line tangent to it are

$$\frac{\text{X} - x}{\varphi' z} = \frac{\text{Y} - y}{\varphi' z\,(\tan s - \sec s.\,\theta' v)} = \frac{\text{Z} - z}{\sec s.\,\theta' v} \quad . \quad . \quad \text{(F)}$$

The stability of the structure demands, that the line whose equations are (F) be perpendicular to that whose equations are (D), therefore the condition of stability is contained in this equation,

$$\left.\begin{array}{l} (\varphi' z)^2\,(\sin s - \theta' v) = \theta' v \\ \text{or} \quad \varphi' z = \sqrt{\left\{\dfrac{\theta' v}{\sin s - \theta' v}\right\}} \\ \text{or} \quad \theta' v = \sin s\,\dfrac{(\varphi' z)^2}{1 + (\varphi' z)^2} \end{array}\right\} \quad . \quad . \quad . \quad \text{(G)}$$

The last form may also be put thus:

$$\frac{\delta u}{\delta v} = \sin s \ \frac{d u^2}{d u^2 + d z^2}$$

in which the characteristic δ refers to the joint, d to the line of pressure. But $\frac{d u^2}{du^2 + d z^2}$ is the square of the cosine of the inclination of the line of pressure to the horison; whence, if we denote that inclination by i,

$$\frac{\delta u}{\delta v} = \sin s. \cos i^2 \quad . \quad . \quad . \text{(H)}$$

When, then, as is the case at the crown of the arch, i is zero, $\frac{\delta u}{\delta v} = \sin s$; but $\frac{\delta u}{\delta v} = -\frac{\delta y}{\delta v} + \sin s$, so that, at the crown, $\frac{\delta y}{\delta v} = o$, that is, the horizontal projection of the joint, is there perpendicular to the parapet, as might easily have been anticipated; but when i increases, its cosine decreases, and therefore $\frac{\delta y}{\delta v} = \sin s. \sin i^2$ (I) must increase: that is, the line must bend away, from being perpendicular to the parapet, until, if i could reach 90°, it would be parallel to the abutment.

Since $\frac{\delta v}{\delta x} = \sec s$, the above equation put in rectangular co-ordinates becomes,

$$\frac{\delta y}{\delta x} = \tan s. \sin i^2 \quad . \quad . \quad . \text{(K)}$$

If a be taken to represent the arc of which u is the projection, $\cos i = \frac{du}{da}$ and equation H becomes,

$$\frac{\delta a}{\delta v} = \sin s. \cos i \quad . \quad . \quad . \text{(L)}$$

and thus, if we imagine two joints running quite close to each other, cutting the crown-line at the minute distance δv, the distance δa, intercepted between them on the arc, or the breadth of the course, is proportional to cosine i.

The above equation can also be put under the form

$$\frac{\delta a}{\delta x} = \tan s. \cos i \quad . \quad . \quad . \quad \text{(M)}$$

Again, we have $\frac{\delta u}{\delta z} = \cot i$; whence equation H becomes,

$$\frac{\delta z}{\delta v} = \sin s. \sin i. \cos i = \tfrac{1}{2} \sin s. \sin 2\, i. \quad . \quad . \text{(N)}$$

$$\frac{\delta z}{\delta x} = \tan s. \sin i. \cos i = \tfrac{1}{2} \tan s. \sin 2\, i. \quad . \quad . \quad . \text{(O)}$$

From which it will be seen, that the general statement made as to the side elevation of the joint is true.

Lastly, we have

$$\frac{\delta y}{\delta z} = \sin s \ \frac{\delta v}{\delta z} - \frac{\delta u}{\delta z} = \tan i = \frac{\delta z}{\delta u} \quad . \ . \ . \quad \text{(P)}$$

whence it is, that the end elevation of the joint crosses that of the line of pressure at right angles.

Before proceeding to apply the above differential equations to particular cases, the following recapitulation may be made:

Equation H gives the Horizontal Projection.
... L ... Developement.
... O ... Side Elevation.
... P ... End Elevation of the Joint.

And it is to be remarked, that these equations are absolutely general, applying to every skewed cylindroid arch.

Having now completed the general investigation, I proceed to apply the principles to specific cases; in the first case to the circular arch.

Denoting by r the radius of the circle, we have

$$i = \frac{a}{r}, \ z = r \cos \frac{a}{r}, \ u = r \sin \frac{a}{r}; \ z^2 + u^2 = r^2;$$

equations which take the place of (B) in the general analysis.

For the horizontal projection of a joint we have

$$\frac{\delta u}{\delta v} = \sin s \left(\cos \frac{a}{r}\right)^2 = \sin s. \frac{r^2 - u^2}{r^2}$$

and thus

$$\frac{\delta v}{\delta u} = \operatorname{cse} s. \frac{r^2}{r^2 - u^2};$$

whence integrating

$$v = r. \operatorname{cse} s. \text{ nep. log} \sqrt{\left(\frac{r+u}{r-u}\right)}$$

Now $v' = r. \operatorname{cse} s. \text{ nep. log } (r+u)$ is the equation of a logarithmic curve to oblique co-ordinates having one side of the semicylinder for its axis, and $r \operatorname{cse} s.$ for its subtangent: while $-v'' = r. \operatorname{cse} s. \text{ nep. log } (r-u)$ is that of a similar curve having the other side of the semi-cylinder for its asymptote, and thus the v of the joint which is the arithmetical mean of these is obtained by bisecting the interval between the two logarithmics.

Passing to common logarithms, and putting M for the modulus, .43429448, etc. we have

$$v = \frac{r. \operatorname{cse} s}{2\,\mathrm{M}} \log \frac{r+u}{r-u}$$

$$u = r \, \frac{10^{\frac{2\,\mathrm{M}\,v}{r \operatorname{cse} s}} - 1}{10^{\frac{2\,\mathrm{M}\,v}{r \operatorname{cse} s}} + 1}.$$

The horizontal projection of the joint of a circular skewed arch is thus a new curve to which I have given the name of Double Logarithmic: the

analogy between this curve and the common catenary has already been pointed out.

In order to trace the side elevation, we must resume equation (O) which, when adapted to the circular arch, is

$$\frac{\partial z}{\partial x} = \tan s. \frac{z}{r} \sqrt{\left(\frac{r^2 - z^2}{r^2}\right)} \qquad \text{whence}$$

$$x = \frac{r. \cot s}{z} \text{nep. log} \frac{r + \sqrt{r^2 - s^2}}{r - \sqrt{r^2 - z^2}}$$

$$= \text{nep log } 10. r. \cot s \log \tan \left(45° + \frac{a}{z\, r}\right)$$

But the equation

$$x' = \frac{r}{z} \text{ nep. log} \frac{r + \sqrt{r^2 - z^2}}{r - \sqrt{r^2 - z}} - \sqrt{r^2 - z^2}$$

is just the equation of the tractory, whence

$$x'' = \frac{r}{z} \text{ nep. log} \frac{r + \sqrt{r^2 - z^2}}{r - \sqrt{r^2 - z^2}}$$

is the equation of a curve having its ordinates greater than those of the tractory by the quantity $\sqrt{r^2 - z^2}$, this curve I have named the *companion to the tractory*, or, on account of the connection which is explained in the paper, and which at once flows from the above, the *inverted catenary*.

The equation for the end elevation of a joint adapted to the circular arch is

$$\frac{\partial y}{\partial z} = \sqrt{\left(\frac{r^2 - z^2}{r^2}\right)} \qquad \text{whence}$$

$$-y = \text{nep. log} \sqrt{\left\{\frac{r + \sqrt{r^2 - z^2}}{r - \sqrt{r^2 - z^2}}\right\}} - \sqrt{r^2 - z^2}$$

which is the well known equation of the tractory. This is the characteristic curve of the circular oblique arch: as all tractories are similar to each other, it is easy to make a table of its co-ordinates.

The preceding equations enable us to obtain any one of the projections of the joint, and are essential to a knowledge of the nature of the different curves. They are, however, inconvenient when we wish to ascertain the dimensions of the individual arch-stones, and need, for that purpose, to know the intersection of the joint with any one of the lines of pressure. The equation of the development furnishes us with the means of obtaining these points, as well as all the projections, by processes remarkable for their simplicity. To find this equation I resume (L) which, adapted to the circular arch, becomes

$$\frac{\delta v}{\delta a} = \operatorname{csc} s. \sec \frac{a}{r} \qquad \text{whence}$$

$$v = r. \operatorname{cse} s. \operatorname{nep.log} \tan \left(\frac{\pi}{4} + \frac{a}{2r}\right)$$

or, observing that $\frac{a}{r} = i$, and passing to common logarithmic tables,

$$v = \operatorname{nep.\ log}\ 10.\ r \operatorname{cse} s. \log \tan \left(45^\circ + \frac{i}{2}\right)$$

whence by inversion

$$\log \tan \left(45^\circ + \frac{i}{2}\right) = \frac{\text{M. sin } s}{r} . v.$$

from which the values of i can be very easily found; especially when they correspond to equi-different values of v.

The expert computer will now perceive at a glance, that all the operations needed to determine the co-ordinates of the various points may now be arranged in a simple tabular form so as to require scarcely any figuring.

I now proceed to the Elliptic Oblique Arch. Put r for the horizontal and ϱ for the vertical radius; the equation of the curve then becomes

$$\frac{u^2}{r^2} + \frac{z^2}{\varrho^2} = 1$$

which takes the place of (B).

This equation may also be put under the form

$$u = r \sin \alpha, \quad z = \varrho \cos \alpha.$$

where α is the inclination of the trammel bar that would trace out the ellipse; from this we find

$$\frac{\delta v}{\delta \alpha} = \frac{\operatorname{csc} s}{r} \left\{ (r^2 - \varrho^2) \cos \alpha + \varrho^2 \sec \alpha \right\} \qquad \text{whence}$$

$$v = \frac{\operatorname{csc} s}{r} \left\{ (r^2 - \varrho^2) \sin \alpha + \varrho^2 \operatorname{nep\ log} \tan \left(45 + \frac{\alpha}{2}\right) \right\}$$

Otherwise we obtain

$$\frac{\delta v}{\delta u} = \frac{\operatorname{csc} s}{r} \left\{ (r^2 - \varrho^2) + \varrho^2 \frac{r}{r^2 - u^2} \right\}$$

$$v = \operatorname{cse} s \left\{ \frac{r^2 - \varrho^2}{r^2} u + \frac{\varrho^2}{r} \operatorname{nep\ log} \sqrt{\left(\frac{r+u}{r-u}\right)} \right\}$$

At first glance it might be thought that this equation gives a new curve; it is, however, still a double logarithmic, having its parts determined in the manner already described.

To find the side elevation we have

$$\frac{\delta z}{\delta r} = \tan s \frac{-r \varrho z \sqrt{\varrho^2 - z^2}}{\varrho^4 + (r^2 \quad \varrho^2) z^2} \qquad \text{whence}$$

$$x = \cot s \left\{ \frac{r^2 - \varrho^2}{r\,\varrho} \sqrt{\varrho^2 - z^2} + \frac{\varrho^2}{r} \log \sqrt{\left(\frac{\varrho + \sqrt{\varrho^2 - z^2}}{\varrho - \sqrt{\varrho^2 - z^2}} \right)} \right\};$$

it is, however, more easily determined thus

$$\frac{\delta x}{\delta z} = \cot s \, \frac{r^2 + \varrho^2 \tan \alpha}{r\,\varrho \tan \alpha}$$

$$= \cot s \left\{ \frac{r}{\varrho} \cot \alpha + \frac{\varrho}{r} \tan \alpha \right\}$$

But $\delta z = -\varrho \sin \alpha . \delta \alpha$. whence

$$x = -\cot s \left\{ \frac{r^2 - \varrho^2}{r} \sin \alpha + \frac{\varrho^2}{r} \text{nep. l. tan} \left(45 + \frac{\alpha}{2} \right) \right\}$$

For the end elevation we have recourse to equation (P) which gives

$$\frac{\delta y}{\delta \alpha} = \frac{\varrho^2}{r} \left\{ \sec \alpha - \cos \alpha \right\} \quad \text{and thus}$$

$$y = \frac{\varrho^2}{r} \left\{ \text{nep. log tan } (45 + \tfrac{1}{2} \alpha) - \sin \alpha \right\}$$

which is the equation of the tractory modified by the existence of the factor $\frac{\varrho}{r}$. From this equation the determination of the individual points is most easily obtained.

I now proceed to consider the Parabolic Arch. f being the focal distance, the equation of the parabola is

$$u^2 = 4fz, \text{ whence } u\,du = 2f\,dz$$

whence again the equation

$$v = \operatorname{cosec} s \left\{ u + \frac{u^3}{12 f^2} \right\}$$

which belongs to the horizontal projection ; also

$$x = \cot s \left\{ 2u + \frac{u^3}{6f^2} \right\} \quad \text{or}$$

$$x = \cot s \cdot \frac{2}{3} \sqrt{fz} \left\{ 3 + \frac{z}{f} \right\} :$$

and also

$$y^2 = \frac{4}{9} \frac{z^3}{f}$$

which are the equations of the three projections.

I have now run over the equations which serve to determine the different parts of oblique, circular, elliptic, and parabolic arches, and had intended to supply examples of the requisite calculations ; but after proceeding to some length in this, it occurred to me that those who have followed the preceding investigations stand in no need of such illustrations, and that these, therefore, would merely occupy room without being productive of any benefit.

16*th May* 1838.

Lord Gray's Meteorological Table for 1839.

Extracted from the Register kept at Kinfauns Castle, North Britain.
Lat. 56° 23′ 30″.—Above the level of the sea, 150 feet.

1839.	Morning, ¼ past 9.		Evening, ¼ past 8.		Mean Temperature by Six's Thermometer.			Depth of Rain in Gard.	No. of days.	
	Mean Height of		Mean Height of						Rain or Snow.	Fair.
	Barom.	Therm.	Barom.	Therm.	Lowest.	Highest.	Mean.			
January, .	29.598	35.677	29.640	32.710	29.065	38.871	33.968	1.35	8	23
February,	29.659	36.893	29.590	35.643	30.679	42.107	36.393	1.56	11	17
March, . .	29.679	38.322	29.659	35.387	31.193	43.193	37.193	2.55	18	13
April, . . .	29.953	44.200	29.943	40.333	34.833	49.300	42.066	1.08	6	24
May, . . .	29.870	50.161	29.899	44.774	38.226	57.064	47.645	.90	7	24
June, . . .	29.737	56.366	29.750	52.000	46.033	63.566	54.800	2.80	15	15
July, . . .	29.655	58.253	29.634	54.677	49.548	65.806	57.677	4.27	19	12
August, . .	29.776	57.226	29.760	52.774	47.807	63.419	55.613	1.68	16	15
September,	29.346	53.366	29.375	49.266	44.466	59.533	52.000	5.85	20	10
October, . .	29.895	46.710	29.919	44.258	40.516	52.968	46.742	3.10	16	15
November,	29.566	42.266	29.574	41.000	37.500	46.566	42.033	3.41	17	13
December,	29.525	37.710	29.530	36.129	34.161	41.581	37.871	3.68	19	12
Average of the year,	29.688	46.429	29.689	43.246	38.669	51.997	45.333	32.23	172	193

ANNUAL RESULTS.

MORNING.

	Barometer.			Thermometer.	
	Observations.		*Wind.*		*Wind.*
Highest, .	10th April, .		N. 30.57.	19th June, .	SW. 66°.
Lowest, .	7th January, .		W. 27.75.	21st February,	NE. 23°.

EVENING.

Highest, .	10th April, .		NE. 30.62.	20th July, .	S. 60
Lowest, .	1st September,		N. 28.56.	29th January, .	W. 20°

Weather.	*Days.*	*Wind.*	*Times.*
Fair, ...	193	N. and NE. .	39
Rain or Snow, ...	172	E. and SE. . .	108
	365	S. and SW. . .	117
		W. and NW. .	101
			365

Extreme Cold and Heat by Six's Thermometer:—

Coldest, . .	21st February, . .	Wind NE. .	14°.
Hottest, . .	3d July, . . .	Wind SW. .	74°
Mean Temperature for the year 1839, ...			45°.333.

Results of two Rain-Gauges.

	In 100.
1. Centre of Kinfauns Garden, about 20 feet above the level of the sea,	32.23
2. Square Tower, Kinfauns Castle, 180 feet, ...	32.71

Mean Results, during each Month for Seven Years, of a Register of the Thermometer, kept at Alford, Aberdeenshire; about Lat. 57° 13′ *N.;* 420 *feet above the Sea, and* 26 *miles inland from the Coast at Aberdeen. Also, the Extremes of both Heat and Cold in each Month, the Mean of each Year, the Mean from April to September both inclusive, and from July to September both inclusive, and the quantity of Rain that fell in the five last Years, with the Fair and Rainy Days.* By the Rev. JAMES FARQUHARSON, LL.D., F.R.S.

The Thermometer was registered at 9¼ a.m. and 8½ p.m.

1833 THERMOMETER.						1834				
	9¼ A.M.	8½ P.M.	Mean.	Highest.	Lowest.	9¼ A.M.	8½ P.M.	Mean.	Highest.	Lowest.
	°	°	°	°	°	°	°	°	°	°
January, .	28.07	30.09	29.08	44.	11.	38.3	36.8	37.55	52	23
February,	35.07	35.14	35.10	46.	21.	38.39	38.78	38.58	55	24
March, . .	36.79	35.33	36.06	45.	26.	41.67	39.51	40.59	57	20
April, . .	44.46	41.25	42.85	62.	27.5	45.86	42.96	44.41	56	25
May, . .	56.50	53.87	55.16	72.5	34.	55.61	51.74	53.67	70	21
June, . .	54.58	53.38	53.98	67.	38.	61.13	56.46	58.29	70	36
July, . .	58.0	56.6	57.3	76.	39.	62.45	60.06	61.25	81	42
August, .	53.8	52.12	52.96	64.	40.	59.9	57.67	58.78	74	37
September,	51.95	50.86	51.4	66.	33.	54.03	51.03	52.53	70	31
October, .	46.6	46.42	46.51	61.	28.	47.8	46.77	47.28	62	29
November,	38.57	38.35	38.46	58.	20.	41.76	41.63	41.69	60	20
December,	35.9	36.14	36.02	47.	20.	42.29	40.25	41.27	53	21
	45.024	44.128	44.573			49.01	46.97	47.99		

Mean Temp. of 1833, . . .	°	44.573	Mean Temp. of 1834, .	°	47.99
April to September inclusive, .		52.27	Apr. to Sept., both inclus.,		54.82
July, August, September, inclusive,		53.88	July, Aug., Sept., .		57.52

1835 THERMOMETER.							1836.					
	9¼ A.M.	8½ P.M.	Mean.	Highest.	Lowest.	Rain in Inches.	9¼ A.M.	8½ P.M.	Mean.	Highest.	Lowest.	Rain in Inches.
	°	°	°	°	°		°	°	°	°	°	
Jan. .	34.61	34.55	34.58	52	7	0.75	36.03	36.2	36.115	46	22	3.3
Feb. .	38.57	38.37	38.47	59	24	2.95	35.0	34.3	34.65	46	14	5.25
March,	40.63	37.06	38.845	55	21	3.5	38.40	36.6	37.5	52	13	3.6
April,	45.53	42.13	43.83	59	27	2.375	42.63	39.4	41.015	59	22	3.075
May, .	49.9	47.16	48.53	64	26	2.6	52.83	51.16	51.995	71	30	1.3
June, .	57.76	54.90	56.33	82	35	1.8	57.03	55.6	56.315	74	38	1.65
July, .	59.41	58.48	58.945	73	42	1.6	56.12	55.93	56.025	73	41	5.6
Aug. .	59.93	58.58	59.255	79	40	2.275	55.19	54.54	54.865	69	31	2.35
Sept. .	53.2	51.23	52.215	66	35	5.2	49.86	48.73	49.295	60	31	5.6
Oct. .	44.25	43.19	43.72	56	26	8.35	45.38	44.54	44.96	62	25	4.9
Nov. .	41.03	41.33	41.18	52	21	4.5	37.36	37.28	37.32	48	26	5.575
Dec. .	35.45	35.13	35.29	47	12	1.8	36.12	36.9	36.51	51	11	3.35
	46.689	45.1758	45.9325			37.7	45.1625	44.265	44.713			45.55

Mean Temperature of 1835, . .	°	45.9325	Mean Temp. of 1836, . . .	°	44.713
April to September, inclusive, .		53.183	April to Sept. inclusive, .		51.592
July, August, September, . . .		56.8	July, Aug., Sept., ...		53.36

Tables of Mean Results, &c. (Continued.)

THERMOMETER.

1837.	9¼ A.M.	8½ P.M.	Mean.	Highest.	Lowest.	Rain in Inches.
	°	°	°	°	°	
January, . .	35.19	34.19	34.69	45	18	2.3
February, .	37.03	36.39	36.71	50	25	3.3
March, . .	33.9	31.96	32.93	46	8	1.25
April, . .	39.18	37.23	38.205	54	20	1.75
May, . .	47.38	45.83	46.605	60	32	1.8
June, . .	56.5	55.96	56.23	77	28	1.9
July, . .	58.87	58.41	58.64	69	44	7.8
August, . .	55.25	55.83	55.54	76	33	4.9
September, .	51.56	51.8	51.68	64	38	2.75
October, . .	48.93	48.38	48.655	60	25	.7
November, .	38.13	37.46	37.785	57	25	1.45
December, .	38.9	39.32	39.11	57	22	2.15
	45.068	44.396	44.73			32.05

Mean Temp. of 1837, 44°.73. April to Sept. inclusive, 51°.15. July, August, Sept. 55°.252.

1838.	9¼ A.M.	8½ P.M.	Mean.	Highest.	Lowest.	Rain in Inches.
	°	°	°	°	°	
January, . .	28.74	28.48	28.61	46	9	3.0
February, .	27.785	27.357	27.571	44	—12	3.5
March, . .	37.258	35.838	36.548	52	13	5.4
April, . .	39.3	38.33	38.815	54	19	3.775
May, . .	49.61	46.41	48.01	69	21	2.7
June, . .	55.0	53.7	54.35	66	24	4.2
July, . .	58.7	58.61	58.655	74	43	1.2
August, . .	55.645	54.774	55.2095	67	34	3.025
September, .	51.4	50.4	50.9	66	28	4.6
October, . .	43.741	43.967	43.854	57	15	2.85
November, .	35.9	36.33	36.115	48	18	4.9
December, .	38.645	38.32	38.482	51	21	2.1
	43.477	42.7096	43.0933			41.25

Mean Temp. of 1838, 43°.0933. April to Sept. inclus., 50°.9899. July, Aug., Sept., 54°.9215.

1839.	9¼ A.M.	8½ P.M.	Mean.	Highest.	Lowest.	Rain in Inches.
	°	°	°	°	°	
January, . .	32.87	33.16	33.015	48	17	3.15
February, .	35.1	36.36	35.73	46	11	1.0
March, . .	35.9	34.45	34.175	45	16	2.0
April, . .	43.3	40.46	41.88	58	12	0.65
May, . .	48.16	45.64	46.9	65	19	3.35
June, . .	55.56	54.16	54.86	75	25	3.0
July, . .	58.8	56.96	57.88	68	38	3.6
August, . .	56.32	54.06	55.19	68	40	1.925
September, .	52.8	50.96	51.88	60	36	4.725
October, . .	45.39	46.45	45.92	56	26	2.6
November, .	39.86	38.73	39.295	51	7	4.0
December, .	36.58	36.48	36.53	50	21	6.3
	45.054	43.988	44.521			36.3

Mean Temp. of 1839, 44°.521. April to Sept. inclus., 50°.98. July, August, Sept., 54°.98.

Tables of Mean Results, &c. (Continued.)

RECAPITULATION OF MEANS, AND QUANTITY OF RAIN.

Years.	Mean Temp. of Years.	Mean Temp. of Apr. to Sept. both inclus.	Mean Temp. of July, August, September.	Rain in Inches.
1833	44.573°	52.27°	53.88°	
1834	47.99	54.82	57.52	
1835	45.93	53.183	55.8	37.7°
1836	44.713	51.592	53.36	45.55
1837	44.73	51.15	55.252	32.05
1838	43.0933	50.9c99	54.9215	41.25
1839	44.521	50.98	54.98	36.3
Mean of 7 Years.	45.0784	52.1407	55.2447	38.57 Mean of 5 Years.

FAIR DAYS, AND DAYS OF RAIN OR SNOW, FOR FIVE YEARS.

	Fair Days.	Rain or Snow, more or less
1835,	233	132
1836,	204	162
1837,	239	126
1838,	216	149
1839,	195	170

The highest temperature of the seven years (82°) occurred on June 9th 1835. The lowest of the seven years (—12°) occurred on the morning of the 15th February 1838; but such a low temperature is very uncommon. The thermometer has gone below zero, Fahr., only three times during the last 24 years. One other of the occasions was on the morning of the 3d February 1838, when it fell to —2°.

The crops of 1833, 1836, and 1839, were below the average; that of 1838 greatly deficient, and much injured by the autumnal frosts. That of 1834 was unusually productive in grain.

Register of the Weather, kept at Canaan Cottage, Edinburgh, for 1839.—By ALEX. ADIE, Esq. F.R.S.E., M.W.S., &c.

Months.	Thermometer.				Barometer.		Rain.
	Morn.	Even.	Min.	Max.	Morn.	Even.	
January, .	34.39	31.71	29.03	41.97	29.107	29.094	1.76
February,.	37.64	39.43	31.15	44.75	29.694	29.663	1.45
March, . .	38.42	35.55	31.94	44.55	29.519	29.505	1.47
April, . . .	44.63	40.43	35.10	51.90	29.910	29.891	.33
May, . . .	48.61	45.03	38.42	59.84	29.438	29.798	.47
June, . . .	56.57	50.27	46.83	64.47	29.626	29.663	3.91
July, . . .	60.81	54.74	49.52	67.74	29.547	29.557	3.51
August, . .	58.64	52.39	44.90	65.00	29.735	29.692	1.77
September,	54.89	49.83	44.90	61.59	29.280	29.257	3.09
October, .	48.39	44.61	40.71	54.68	29.777	29.736	2.38
November,	43.50	42.77	36.37	48.10	29.404	29.421	1.65
December,	33.75	38.17	34.19	41.97	29.342	29.300	1.66
Mean of Months,	46.94	43.74	38.59	53.88	29.532	29.548	23.45 Sum.
Mean of the Year,	45.33		46.73		29.540		

Summary of Observations of the Barometer, Thermometer, &c. for 1839, *kept at the Manse of Abbey St Bathan's, Berwickshire, Lat.* 55° 52′, *W. Long.* 2° 23′, *at the height of about* 450 *feet above the level of the Sea.* By the Rev. J. WALLACE, A.M. &c.

1839.	THERMOMETER.						HYGROMETER (Leslie's).						BAROMETER (at 32° of Fahrenheit).						Temperat. of Spring Water.	Mean point of Deposition.	Grains of moisture in a cubic inch of air.	Relative Humidity.	Rain in inches.
Months.	10 A.M.	10 P.M	Mean.	9 A.M.	3 P.M.	Mean.	10 A.M.	10 P.M.	Mean.	9 A.M.	3 P.M.	Mean.	10 A.M.	10 P.M.	Mean.	9 A.M.	3 P.M.	Mean.					
	°	°	°	°	°	°	°	°	°	°	°	°							°	°			
Jan. .	34.7	32.9	33.8	34.1	35.4	34.7	7	3	5	5	7	6	29.195	29.228	29.211	29.190	29.201	29.195	45.5	30.	.00125	887	1.689
Feb. . .	36.2	34.8	35.5	35.1	37.8	36.5	5	2	3	4	6	5	29.284	29.213	29.249	29.276	29.238	29.257	45.7	33.	.00137	913	1.095
March,	37.3	33.7	35.5	36.3	38.4	37.3	7	3	5	6	9	8	29.274	27.260	29.267	29.263	29.261	29.262	46.	32.	.00134	900	2.324
April, .	43.4	38.2	40.8	42.2	44.4	43.3	14	5	9	12	17	14	29.574	29.585	29.579	29.574	29.564	29.569	46.	34.7	.00148	822	0.499
May, . .	47.5	41.6	44.5	46.5	50.1	48.3	18	11	15	17	21	19	29.465	29.478	29.472	29.465	29.450	29.457	46.	36.2	.00154	758	1.125
June, .	56.1	48.8	52.4	54.9	56.4	55.6	21	5	13	19	21	20	29.361	29.367	29.364	29.360	29.357	29.359	46.3	47.1	.00219	840	3.547
July, .	58.5	52.0	55.3	57.5	49.0	53.3	15	4	9	14	15	15	29.267	29.271	29.269	29.268	29.253	29.260	46.5	51.7	.00254	891	2.230
Aug. . .	56.3	50.5	53.4	55.0	56.5	55.7	13	2	8	11	14	12	29.384	29.367	29.375	29.382	29.381	29.382	46.5	50.8	.00267	918	1.991
Sept. .	53.9	47.3	50.6	52.5	54.6	53.5	10	1	5	7	10	9	28.983	29.007	28.995	28.979	28.993	28.986	46.	48.	.00224	922	5.086
Oct. . .	47.6	43.4	45.5	45.9	49.4	47.6	7	3	5	6	9	7	29.517	29.520	29.518	29.518	29.499	29.508	46.	43.	.00194	924	4.269
Nov. . .	41.9	39.8	40.8	41.0	43.1	42.1	4	2	3	4	4	4	29.166	29.165	29.165	29.165	29.135	29.150	45.5	39.3	.00169	950	4.445
Dec. . .	36.3	36.2	36.3	35.5	37.1	36.3	2	2	2	2	3	2	29.121	29.107	29.114	29.119	29.106	29.113	45.3	35.	.00147	959	3.645
Means,	45.8	41.6	43.7	44.7	46.0	45.3	10	3	7	9	11	10	29.299	29.297	29.298	29.296	29.286	29.291	46.	40.1	.00179	890	
																			Amount for the Year,...				31.9

Abstract of Meteorological Observations for 1839, *made at Applegarth Manse, Dumfriesshire.* By the Rev. W. DUNBAR, D.D.

Long. 3° 12′ W Lat. 55° 13′ N.; Height above the Sea, 180 feet; Distance from the Sea, 10 miles; Rain-Gauge, 5 feet from the ground. The observations made at 9¼ A.M. and 8½ P.M.

BAROMETER.

1839. Months.	Atmospherie Pressure Morning.	Red. to 32° Fahr., and corrected to sea-level.	Atmospherie pressure. Evening.	Reduced and corrected.	Mean of Morning and Evening.	Reduced and corrected.	Mean Range in the day.	Mean Range in the night.	Mean Range in 24 hours.	Monthly Extremes. Highest.	Lowest.	Greatest Range in 24 hours.	Least Range in 24 hours.
January, . .	29.626	29.790	30.286	30.450	29.956	30.120	0.203	0.174	0.377	30.400	28.062	1.290	.071
February, .	29.660	29.810	29.641	29.790	29.650	29.800	0.220	0.190	0.410	30.202	28.480	1.643	.040
March, . . .	29.441	29.662	29.054	29.195	29.247	29.428	0.105	0.100	0.205	30.100	29.095	.605	.020
April, . . .	29.970	30.108	29.952	30.088	29.961	30.098	0.092	0.080	0.172	30.533	29.110	.450	.010
May, . . .	29.880	29.980	29.892	29.992	29.886	29.986	0.072	0.086	0.158	30.190	29.200	.450	.020
June, . . .	29.765	29.865	29.772	29.872	29.763	29.869	0.078	0.074	0.152	30.150	29.132	.400	.010
July, . . .	29.674	29.767	29.680	29.773	29.677	29.770	0.095	0.083	0.178	30.192	29.124	.540	.030
August, . .	29.793	29.892	29.789	29.888	29.791	29.890	0.091	0.088	0.179	30.200	28.934	.490	.020
September, .	29.392	29.497	29.409	29.514	29.400	29.505	0.090	0.097	0.187	29.950	28.680	.522	.030
October, . .	29.885	30.005	29.895	30.015	29.890	30.010	0.092	0.096	0.188	30.470	29.290	.570	.030
November, .	29.525	29.654	29.533	29.659	29.530	29.656	0.099	0.090	0.189	30.070	29.040	.490	.000
December, .	29.585	29.717	29.658	29.778	29.621	29.747	0.133	0.104	0.237	30.260	29.020	.730	.030
Means,	29.683	30.145	29.713	29.834	29.697	29.823	0.114	0.145	0.219				

THERMOMETER.

1839 Months.	Mean of greatest Heat.	Mean of greatest Cold.	Mean Temperature. Morning.	Mean Temperature. Evening.	Mean of Extremes.	Mean of Morn. and Evening.	Mean of both.	Mean Range of 24 hours.	Monthly Extremes. Highest.	Lowest.	Greatest Range in 24 hours.	Least Range in 24 hours.	Rain in inches.
January, . .	39.50	29.80	33.60	34.70	34.65	34.15	34.40	9.70	48.00	17.50	21.00	2.00	2.44
February, . .	40.70	32.20	35.40	36.50	36.40	35.50	35.90	9.00	46.00	17.00	15.50	0.00	1.57
March, . . .	42.70	32.50	37.80	35.60	37.60	37.20	37.40	10.30	51.00	19.50	18.00	0.50	4.06
April, . . .	47.10	33.70	43.50	41.60	41.90	42.50	42.20	10.40	60.00	27.50	24.50	1.00	0.98
May, . . .	58.20	39.80	51.10	46.90	49.00	49.00	49.00	15.50	69.00	27.50	31.50	6.00	0.70
June, . . .	62.30	47.20	58.10	54.10	54.70	56.10	55.40	15.20	75.00	35.00	29.50	3.00	3.53
July, . . .	63.00	50.50	59.00	57.30	56.70	58.10	57.40	12.60	71.00	40.00	22.50	2.50	6.81
August, . .	60.60	49.90	57.60	55.00	55.20	56.30	55.70	10.50	65.00	35.50	20.50	3.50	4.08
September, .	58.10	46.10	53.00	51.20	52.10	52.10	52.10	11.90	62.00	35.00	20.00	4.50	7.64
October, . .	53.20	41.90	47.80	46.60	47.50	47.20	47.35	11.40	61.00	29.00	21.00	3.50	4.85
November, .	47.70	39.60	43.10	43.20	43.60	43.15	43.37	8.50	56.00	23.00	15.50	2.50	2.96
December, .	38.70	31.40	34.00	35.70	35.05	34.80	34.90	7.30	46.00	21.50	13.50	0.50	3.67
Means,	50.98	39.80	46.16	44.95	45.39	45.55	45.47	11.02					43.29

Dr Dunbar's *Meteorological Observations for* 1839.—(Continued.)

WINDS—THEIR DIRECTION AND FORCE, AND WEATHER; STATING THE NUMBER OF DAYS IN WHICH EACH PREVAILED.

Months.	N.	NE.*	E.	SE.	S.	SW.	W.	NW.	Calm.	Moder.	Brisk.	Strong.	Stormy.	Sun shone	Rain fell.	Snow.	Fair without Sun.	Frost.	Thund.
January,	3	3	—	—	1½	9½	2	12	7	9	5	5	5	17	10	9	14	14	—
Feb. . .	1	—	1	3	3	11	6	3	4	8	8	6	2	21	9	8	7	4	—
March, .	3	1	8	5	3	7	2	2	9	9	8	3	2	25	15	6	6	10	—
April, .	—	4	7½	5	4	5	3	1½	11	7	3	4	5	25	4	4	5	3	—
May, . .	1	9½	2½	1	8½	2	1½	5	11	9	6	2	3	29	4	1	2	2	—
June, . .	2	4	6	3	8	1	6	0	9	12	8	—	1	26	10	—	4	—	2
July, . .	½	1	2	5	10½	8	1	3	14	7	4	3	3	25	17	—	6	—	2
August,	1	5	1	4	4	10	4	2	15	8	4	2	2	27	18	—	4	—	—
Sept. . .	1½	—	3	5	11	3½	6	—	8	11	3	6	2	25	24	—	5	—	1
October,	1½	9	5	5	4	5	1	½	8	11	7	4	1	24	13	—	7	4	—
Nov. . .	1	4	7	5	2	9	2	—	12	10	4	1	3	20	16	2	10	3	—
Dec. . .	—	9	8	2	2	5	2	3	13	6	3	6	3	22	13	3	9	13	—
Total No. of Days,	15½	49½	51	43	61½	76	36½	32	121	107	63	42	32	286	153	33	79	53	5

* Under the Column NE. are ranged all the days in which the wind blew from any intermediate point between N. and E. The same of SE., SW., and NW.

The greatest height of the Barometer was on the 11th of April, when the mercury stood at 30°.530; wind E.NE.: the lowest ditto, on the 19th February, 28°.48. Wind N.

The greatest height of the Thermometer was on the 17th of June, viz. 75°; wind S.SE.: lowest ditto, 1st February, viz. 17°.; wind NW.

The quantity of rain is greater than has fallen any year for at least thirteen years; the mean of that period being 35 inches.

The temperature of the year is 45°.47, nearly two degrees higher than the mean of the three preceding years. It will be observed, that the mean of extremes and the mean of morning and evening coincide so nearly that the difference is only one-tenth and half of a degree. Now, as the mean of extremes is acknowledged to be the nearest approximation to the true temperature, the hours of 9½ a.m. and 8½ p.m., recommended in the Edinburgh Encyclopædia, Art. Thermometer, agreeing in their results so nearly as they do with the mean of extremes, seem to be the most eligible hours for the morning and evening observations.

Results of a Rain-Gauge kept at Carlisle. Communicated by Mr Joseph Atkinson.

Months.	1835.	1836.	1837.	1838.	1839.
January, . .	3.1125	2.9030	2.1995	1.1480	2.9413
February, . .	3.9405	2.1780	2.4340	0.6100	1.7780
March, . . .	2.5875	4.0400	1.8720	2.8758	3.9950
April, . . .	0.7375	1.9765	0.8005	1.5176	0.9400
May, ...	2.2350	0.0400	1.0860	2.0820	0.5450
June, ...	1.3200	3.8430	2.3338	4.5765	2.5770
July, ...	2.9783	6.9145	5.1375	3.1400	3.6500
August, . . .	2.5490	3.8015	3.9000	3.3050	3.6333
September, .	5.2125	4.7065	2.0750	2.1158	4.3185
October, . .	4.3650	2.1790	3.7415	2.9400	2.7041
November, . .	3.3197	2.7143	2.9700	2.8800	2.6000
December, . .	1.9718	3.1473	4.0300	1.0558	1.7975
Totals, . .	34.3293	38.4436	32.6098	28.2465	31.4797

*On the Cultivation of Roses, and the Manufacture of Rose-Water and Attar or Otto, at Ghazeepore.**

I have now the pleasure of sending you the information you require, on the manufacture and trade of rose-water in this district. Ghazeepore seems to have been long famed for its attar and rose-water, and having got the name, it has done its best to preserve it. The cultivation of the rose-plant is sufficient to supply the demand, and as the average remuneration is not more than enough to compensate for the trouble of its culture, no competition from the adjoining districts has been made.

Around the station of Ghazeepore there are about 300 beegahs, or about 150 acres, of ground laid out in small detached fields, as rose-gardens, most carefully protected on all sides

* We are indebted to Dr Jackson, Civil Surgeon at Ghazeepore, for the above very interesting note on the celebrated rose trade of that district. The information was procured for a work now in progress on "Indian Materia Medica;" meanwhile, we have much pleasure in giving publicity in this Journal to the curious facts Dr Jackson has collected.—*Eds. Journal of the Asiatic Society of Bengal. May* 1839.

by high mud-walls and prickly pear fences, to keep out the cattle. These lands, which belong to Zemindars, are planted with rose-trees, and are annually let out at so much per beegah for the ground, and so much additional for the rose-plants —generally five rupees* per beegah, and twenty-five rupees for the rose-trees, of which there are 1000 in each begah. The additional expense for cultivation would be about 8s. 8d., so that for rupees 30s. 8d. you have, for the season, one beegah of 1000 rose-trees.

If the season is good, this beegah of 1000 rose-trees should yield one lac of roses. Purchases for roses are always made at so much per lac. The price, of course, varies according to the year, and will average from forty to seventy rupees. During the past season the latter was the price given for one lac of roses, towards the conclusion.

As soon as the roses come into flower, the Zemindars and cultivators of the rose-gardens, as well as intending purchasers, meet in the city, and, according to the demand and expected produce, a *nerick* is established, and purchasers then enter into agreement with the cultivators for so many lacs of roses at such a price. This agreement is considered binding, and the cultivator is obliged to deliver the quantity at the contract rate; when that is completed, another can be made, but this latter is always at a much higher rate.

The rose-trees come into flower at the beginning of March, and continue so through April. In the morning early the flowers are plucked by numbers of men, women, and children, and are conveyed in large bags to the several contracting parties for distillation. The cultivators themselves very rarely manufacture.

The native apparatus for distilling the rose-water is of the simplest construction. It consists of a large copper or iron boiler, well tinned, capable of holding from eight to twelve gallons, (shaped like the earthen hoondahs in which the Gomastahs send in their opium,) having a large body with a rather narrow neck, and a mouth about eight inches in diameter; on the top of this is fixed the head of the still, which is nothing

* A rupee is equal to about two shillings.

more than an old *dekchee*, or cooking vessel, with a hole in the centre to receive the tube or worm.

The tube is composed of two pieces of bamboo, fastened at an acute angle, and it is covered the whole length with a strong binding of corded string, over which is a luting of earth, to prevent the vapour from escaping. The small end, about two feet long, is fixed into the hole in the centre of the head, where it is well luted with flour and water. The lower arm or end of the tube is carried down into a long-necked vessel or receiver, called a *bhubka*. This is placed in a handee of water, which, as it gets hot, is changed. The head of the still is luted on to the body, and the long arm of the tube in the bhubka is also well provided with a cushion of cloth, so as to keep in all vapour. The boiler is let into an earthen furnace, and the whole is ready for operation.

There is such a variety of rose-water manufactured in the bazar, and so much that bears the name, which is nothing more than a mixture of sandal oil, that it is impossible to lay down the plan which is adopted. The best rose-water, however, in the bazar, may be computed as bearing the proportion of one thousand roses to a seer of water; this, perhaps, may be considered as the best procurable. From one thousand roses, most generally, a seer and a half of rose-water is distilled, and perhaps from this even the attar has been removed.

The boiler of the still will hold from eight to twelve or sixteen thousand roses. On eight thousand roses, from ten to eleven seers of water will be placed, and eight seers of rose-water will be distilled. This, after distillation, is placed in a carboy of glass, and is exposed to the sun for several days, to become *puckah;* it is then stopped with cotton, and has a covering of moist clay put over it; this becoming hard, effectually prevents the scent from escaping. The price of this will be from twelve to sixteen rupees. This is the best that can be procured.

To procure the attar, the roses are put into the still, and the water passes over gradually, as in the rose-water process. After the whole has come over, the rose-water is placed in a large metal basin, which is covered with wetted muslin, tied over to prevent insects or dust getting into it. This vessel is

let into the ground about two feet, which has been previously wetted with water, and it is allowed to remain quiet during the whole night. The attar is always made at the beginning of the season, when the nights are cool. In the morning, early, the little film of attar, which is formed upon the surface of the rose-water during the night, is removed by means of a feather, and it is then carefully placed in a small phial; and day after day as the collection is made, it is placed for a short period in the sun, and after a sufficient quantity has been procured, it is poured off clear, and of the colour of amber, into small phials. Pure attar, when it has been removed only three or four days, has a pale greenish hue; by keeping it loses this, and in a few weeks' time it becomes of a pale yellow. The first few days' distillation does not produce such fine attar as comes off afterwards, in consequence of the dust or little particles of dirt in the still and the tube being mixed with it. This is readily separated from its sinking to the bottom of the attar, which melts at a temperature of 84°. From one lac of roses, it is generally calculated that 180 grains, or one tolah, of attar can be procured; more than this can be obtained if the roses are full-sized, and the nights cold to allow of the congelation. The attar purchased in the bazar is generally adulterated, mixed with sandal oil or sweet oil. Not even the richest native will give the price at which the purest attar alone can be obtained, and the purest attar that is made is sold only to Europeans. During the past year, it has been selling from eighty to ninety rupees the tolah; the year before it might have been purchased for fifty rupees. Native stills are let out at so much per day or week, and it frequently occurs that the residents prepare some rose-water for their own use, as a present to their friends, to secure their being provided with that which is the best. The natives never remove the calices of the rose-flowers, but place the whole into the still as it comes from the gardens.

The best plan appears to me to have this removed, as by this means the rose-water may be preserved a longer time, and is not spoiled by the acid smell occasionally met with in the native rose-water. It is usual to calculate a hundred bottles to one lac of roses. The rose-water should always be twice dis-

tilled. Over ten thousand roses water may be put to allow of sixteen or twenty bottles coming out. The following day these twenty bottles are placed over eight thousand more roses, and about eighteen bottles of rose-water are distilled. This may be considered the best to be met with. The attar is so much lighter than the rose-water, that, previous to use, it is better to expose the rose-water to the sun for a few days, to allow of its being well mixed, and rose-water that has been kept six months is always better than that which has recently been made.

At the commencement of the rose season, people from all parts come to make their purchases, and very large quantities are prepared and sold. There are about thirty-six places in the city of Ghazeepore where rose-water is distilled. These people generally put a large quantity of sandal oil into the receiver, the oil is afterwards carefully removed and sold as sandal attar, and the water put into carboys, and disposed of as rose-water. At the time of the sale, a few drops of sandal oil are placed on the neck of the carboy to give it a fresh scent, and to many of the natives it appears perfectly immaterial whether the scent arises solely from the sandal oil or from the roses. Large quantities of sandal oil are every year brought up from the south, and expended in this way.

The chief use the natives appear to make of the rose-water, or the sandal attar as they term it, is at the period of their festivals and weddings. It is then distributed largely to the guests as they arrive, and sprinkled in profusion in the apartments. A large quantity of rose-water is sold at Benares, and many of the native rajahs send over to Ghazeepore for its purchase. Most of the rose-water, as soon as distilled, is taken away, and after six months from the termination of the manufacture, there are not more than four or five places where it is to be met with.

I should consider that the value of the roses sold for the manufacture of rose-water may be estimated at 15,000 rupees a-year, and from this to 20,000; and from the usual price asked for the rose-water and for which it is sold, I should consider there is a profit of 40,000 rupees. The natives are very fond of using the rose-water as medicine, or as a vehicle

for other mixtures, and they consume a good deal of the petals for the conserve of roses, or *Goolcund*, as they call it. There are several kinds of essential oils produced from the strong scented flowers in this district, which I will procure and send down to you.

Observations on the Temperature of Plants. By M. Van Beck.*

I have repeated the interesting experiments of M. Dutrochet, respecting the proper heat of plants, with complete success, by means of M. Gourjon's galvanometer, and the physiological needles of MM. Becquerel and Breschet.

I selected for my experiments two plants in pots, viz. *Sempervivum spatulatum* and *Sedum cotyledon*, sheltered from the direct action of the solar rays in an apartment of which the ordinary temperature was from 15°.75 to 16°.75 centigrade, and not subjected to any great or sudden changes.

The soldered point of one of the physiological needles, covered with several layers of gum-lac, was inserted into a young leaf of the plant, while the point of the other needle was placed in a similar leaf of the same plant, but dead, its vitality having been destroyed by means of water at a high temperature.

The apparatus being thus disposed, and the needles united by means of conducting wires to the galvanometer, the variable motions of the magnetic needle usually indicating, although in a very irregular manner, the lowest temperature on the side of the living plant, evidently shewed that the phenomenon of the inherent heat was obstructed by the anomalous action of external causes.

The plant was then placed, with a vessel filled with water, under a large glass receiver, having its inside moistened. From the time that a psychrometer placed under the bell-glass, by the side of the plant, indicated that the air was saturated with humidity, the phenomenon of the proper hea manifested itself continually during the whole day.

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* Comptes Rendu, No. 1. 6th Janvier, p. 36.

By presenting distinct and daily periods, I perceive that this heat gradually augments till the afternoon, when it begins to diminish, almost entirely disappearing during the night, and shewing itself again on the following day.

The maximum of inherent heat which I observed in this manner, on the 29th September, an hour and a quarter after noon, in a young leaf of *Sedum cotyledon*, tested by numerous series of comparative trials, did not appear to me much to exceed 0°.25 centigrade, while it was usually much less. In rainy and dull days, the phenomenon did not display itself so evidently as when the weather was calm and clear.

Up to this point my experiments perfectly agree with those of M. Dutrochet; but in one respect a difference appeared which it is important to specify.

M. Dutrochet states himself to have found that, in atmospheric air not saturated with humidity, the dead leaf was always found colder than a similar leaf of a living plant; while my experiments, although repeated in many different forms, have constantly presented to me the greatest cold in the living plant.

This result did not appear to me singular, for, seeing that the cold which manifests itself on the surface of living plants is certainly produced by evaporation, and that this evaporation is at the same time in intimate relation with their vital functions,* it was to be presumed that, notwithstanding the proper heat, which is always very small, of the living plant, the latter would shew itself in a free air, where the evaporation might still proceed, at all times *colder* than the dead leaf, since in the latter evaporation could not take place but by ordinary physical causes, while it is augmented by the vital action in the living plant.

A singular phenomenon, which I have always observed in the course of my experiments, is, that in suddenly raising the

* This relation shewed itself particularly in the experiments which I made some time ago, in connection with Professor Bergsma, on a hyacinth in flower, with its roots plunged in warm water, a report of which we have had the honour to communicate to the Academy.

bell-glass which cut off all communication between the air of the apartment and that of the plant, the heat of the latter always rose suddenly some tenths of a degree. This phenomenon, however, lasted only a few minutes; the magnetic needle soon retrograded, passing zero of the scale, and shewing by its opposite and permanent deviation, that the living plant had a much lower temperature than the dead leaf, as is always the case in the atmosphere.

Is this phenomenon to be ascribed to the instantaneous access of the free air to the plant, which, by stimulating its vital functions, which were depressed by its having been kept in a less pure air, augments at the same time its proper heat, *before the counteracting and frigorific influence of re-established evaporation has had time to make itself felt?*

This I cannot venture to decide; but I hope that other philosophers and naturalists will engage in these researches, which, if I am not deceived, may yet throw light on many an interesting question in vegetable physiology.*

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* *Note occasioned by the Observations of M. Van Beck, on the proper Temperature of Plants.* By M. DUTROCHET.

I ought to return my thanks to M. Van Beck, for the eagerness he has shewn in repeating my experiments on the internal heat of vegetables. His verification of the existence of this heat, and its diurnal period, henceforth places these facts in the number of those definitively established in science, which in general admits only of such as have been seen by more than one observer.

M. Van Beck differs from me in regard to one matter of very little importance. I have stated that when making a comparative trial in the open air of a living vegetable, and another of a similar kind in a dead state, that the latter always appeared colder than the former; M. Van Beck always obtained an opposite result. This difference in the result of our observations is perhaps owing to a difference in the mode of preparing the experiment. M. Van Beck, like myself, plunged the part of the vegetable he wished to kill into very warm water; perhaps he then left it to cool in the open air, and thus lose by evaporation a part of the water which moistened its surface. In my case, in was cooled by immersion in cold water, and in this condition, completely saturated, it was subjected to experiment. It may readily be conceived, that there must have been more evaporation in it than in the less humid living vegetable, and that, consequently, it must

On the Examination of Mountains and Plains, with the view of discovering previously unknown Repositories of useful Minerals. By the late FREDERICK MOHS, Councillor of Mines at Vienna, and Knight of the Royal Saxon Order of Civil Merit, &c.*

When we enter a mountainous district or a plain, for the purpose of searching for repositories of useful minerals, worthy of engaging the attention of the miner, it is essential that we should become acquainted with its precise position and bearings. With this view, the first thing to be done, is to ascertain from good maps, the principal direction of the mountains and the chief limits of the plains, so that we may in every case be able to determine exactly the point where we are, that is, that we may be prepared to judge of the position of that point in relation to the whole district. It is also very advisable, before proceeding to the actual examination, to delineate a map on a large scale, inasmuch as the largest topographical maps are not sufficient to afford the investigator all the assistance which may be expected from this source. This enlarged map is as it were the magazine in which we are to deposit all the observations made during the actual survey, in order that, at the termination of the examination, we may borrow from it data for the preparation of correct petrographical or geognostical maps and sections. Most representations of this kind are so deficient in exactness as not to be available for so important an undertaking as the one we are now considering.

When in possession of such a map, the next operation is, to seek out and ascend the most important and remarkable emi-

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have been colder than the latter; while an opposite result might be obtained when the vegetable, killed by warm water, had exhaled its excess of moisture, by remaining for a time in the open air. Perhaps also the particular parts of the vegetables used may have had an influence on the different results in question.—*Comptes Rendus, January* 13. 1840.

* Translated from an interesting little work entitled "*Anleitung Zum Schürfen,*" published in Vienna in 1838, by order of the Imperial Chamber of Mines, &c.—EDIT.

nences; and in doing this the following particulars are to be carefully noted: *First,* the height above the sea, which is to be determined barometrically; and also the height above the foot of the mountain, and above those points of the valleys running towards the eminences, at which they begin to ascend more or less rapidly. The points at which the measurements are made, must of course, be accurately entered on the map. *Secondly,* the mountain-rocks must be fully examined, their nature must be determined, and well-selected specimens collected. The direction and dip of the structure-surfaces, or in other words what is termed the stratification in slaty mountain masses, must be determined at as many points as possible; all considerable changes in direction and dip must be indicated on the map; and even the smaller bendings must not be left unnoticed. Farther, the alternation of the mountain masses, when such occurs, must be observed, and also the appearances these masses present at their junction, in respect to the changes which the particular rocks themselves, or the mountain-masses, undergo under such circumstances; the passages that occur, and the foreign substances which separate two masses of rock from each other at their junction must be minutely examined; and finally, it is necessary to attend to the minerals which do not enter into the composition of the mountain-rocks themselves, such as garnet, tourmaline, &c., but which occur in one or the other of the neighbouring masses, either near or at least at no great distance from their common junction. All such observations, however unimportant they seem, may, when combined with others, eventually become of moment, and may, at all events, serve or contribute to enable us to recognise the mountain-masses that have been examined in more remote places, an assistance which is attended with much advantage in our researches. *Thirdly,* We ought from these high points to endeavour to obtain as connected a general view as possible of the surrounding districts, which gradually may produce a representation of the whole mountains, that will be at least much more lively, and much more calculated to enable us to draw conclusions, than any which could be obtained directly from the map. The features of the principal chain, and of the lateral chains, the

position of the points in these which are most remarkable, either from their height, their external form, or constitution: and also the situation of the valleys and ravines, together with their connection with one another, are important objects of attention. Opportunities will not be awanting of perfecting the map, either by adding phenomena worthy of observation, which the small scale of the original did not allow of being represented, or by endeavouring to correct, as far as is permitted by the circumstances, or is necessary for our object, little errors which are almost unavoidable in representations of this kind, and which can only be noticed by the geologist. It appears advisable to bring into connection what has now been pointed out, and completely to finish it before beginning the actual search for deposits worthy of being excavated by mining operations. In this manner, while, on the one hand, we obtain the most accurate and best preparatory introduction to this search; on the other hand, during that search, we are not disturbed or distracted by such observations, for example, by barometrical measurements, and are therefore not necessitated to carry fragile instruments every where with us, which might perhaps prevent us from ascending rocks or following paths where personal safety would require all our attention, and where we might not be able to bestow attention on other things besides the hammer. It must here be remarked, then, when we find an opportunity of making observations of any kind on our road to the above-mentioned high points, in the valleys, or on the acclivities of the ridges, we should take advantage of them even when we know previously that we are again to pass over the same ground; for in investigations, which, when they are successful and are not dependent on accident, are combined with so many difficulties as the discovery of repositories of useful minerals that would repay the expense of mining operations, we ought to pass over nothing which tends to advance them, and to diminish the arduous nature of the undertaking; and this is done by every accurate observation.

After we are in possession of this general view, we are prepared to sketch out a proper plan for the actual investigation in aid of the discovery of repositories of useful minerals. Such

an investigation must go into detail; but, at the same time, it must be undertaken in such a manner that a general result shall follow: that is, we must not only be careful that nothing of any consequence escapes us, but also that every thing shall be brought into proper connection. A well imagined plan will render very essential aid, at all events in saving time and expense; but this cannot be executed without an accurate and special knowledge of the district under observation, and this can only be acquired by the mode which I have pointed out. In general, it is most advantageous to begin with a longitudinal valley, and to select the largest and most extensive; one also which cuts through the mountains, and in which the transverse valleys terminate that present themselves on the declivities of these mountains. But here there are two difficulties in the way, which, in many cases, are not without importance, and which must be attended to. The bottoms of these broad and open valleys are, for the most part, covered by cultivation, and the river and roads occupy a considerable portion of their surface. Natural and artificial causes have also, frequently, so altered the acclivities of these valleys, that they present but little opportunity of making observations; and the only point of view in which we can make use of them, under such circumstances, consists in this, that we collect information regarding discoveries which have been made accidentally during the construction of roads, houses, fortifications, or wells, during quarrying operations, &c., or in consequence of great natural occurrences. Not one of the quarries of the district should remain unexamined; but the information collected must be used with caution, and we must endeavour to convince ourselves of its truth or probability, by weighing carefully the circumstances and relations which we can ourselves observe. The second difficulty is of much greater moment; the longitudinal valleys, in slaty mountains, frequently run in one and the same mountain mass, and follow its direction without cutting its structure at considerable angles; or, what is always more frequent, they run along the boundary of two mountain-masses, in such a manner that one of the declivities consists of the one mountain-mass, and the opposite of the other. It is not without

example, although the occurrence is comparatively a rare one, that the rocky masses cross over from one side to the other. The longitudinal valleys, therefore, present but little opportunity of making observations; and hence the last-mentioned points are in general the most important, at least in slate mountains.

It follows from these considerations that, in the above-mentioned mountain-masses, the longitudinal valleys, as they neither cut across the stratification, nor the structure of the masses which constitute the district, at considerable angles, are not particularly suited to geological investigations; and hence, that the openings of the transverse valleys are the places which, in such valleys, are more especially to be investigated. In making this remark, however, we must except particular basin or trough shaped widenings, which extend from the bottoms of such valleys towards their sides, and in which we not unfrequently find peculiar formations, such as coal tracts, &c.; these are easily recognised by the rocks of which they are composed, and are with facility distinguished from the mountain-masses which actually form the sides of the valley.

We must not, however, be understood to mean, that the remaining portions of the longitudinal valleys are to be passed over unexamined. Nothing should remain unexamined, for discoveries are often made where they are not anticipated or surmised; and although we know that there is but little alternation of rocks, and therefore but little opportunity of discovering the outcrop of beds, yet we do not know but that veins may run through the longitudinal valley, whose outcrop, as they for the most part cut the structure of the mountain-masses and the direction of the stratification at large angles, is more easily discovered there than in the transverse valleys. It is self-evident how we are to apply what we have already said, and part of what follows, to valleys, the rocks composing which possess no slaty structure.

Notwithstanding this, in the examination of mountains, or a mountainous district, with the view of discovering available repositories of useful minerals, our attention is more particularly directed to the transverse valleys, which, at least in longi-

tudinal ranges, are parallel or nearly so, and which generally terminate in the longitudinal valleys, or in plains at the foot of the mountains, cutting, for the most part, almost at right angles the direction of the mountain-ranges, and the stratification and structure of the mountain-masses. Whoever has studied attentively the relations of the surface and the relations of the stratification of mountains in nature (not according to any system merely found in books), knows what modifications and deviations are to be expected in respect to the right-angled union of the descending lateral chains with the principal chain, and in respect to the parallel position of the valleys, and hence this requires no further consideration ; but another character seems not to be superfluous in this place, which is, that we must not from what we have observed on one declivity, in reference to the relations we have been discussing, form our conclusions as to what may be presented by the other, without having previously seen and examined it ; because the two declivities of a mountain-range are not unfrequently much more different, in every respect, than certain assumptions, especially the precipitation theory, generally represent them to be.

As to the transverse valleys, although not one of them ought to remain uninvestigated, yet a proper selection must be made, in order that we may commence with that one which promises to afford the most information, and which is therefore likely to assist and facilitate our search to the greatest extent. The selection of such a transverse valley can only be made by those who, in the manner above described, have acquired a perfect and personal knowledge of the whole district. In general, it may be anticipated that such a valley will be found in the centre of the mountains, and will be that one which penetrates most deeply into their mass, and which reaches farthest into the high mountain-ranges ; which cuts the stratification and rocky structure most nearly at right angles, and whose declivities present the most numerous and best opportunities of making observations by means of the numerous exposures on their sides, and also by their lateral valleys and ravines. These are all relations which can be easily perceived, and which must be well considered before we be-

gin the actual search, which often requires a considerable time if it is to be decisive.

When the bottom of such a valley is not obscured by cultivation near its mouth, or generally in its lower portions, and when the river allows of its channel being examined, such are the most advisable points with which to make a beginning in our examination. In particular, the loose masses in the streams are, as it were, an index of the composition of the valley; for, not only do we ascertain from them what rocks are to be expected, but we have also our attention directed to the occurrence of particular repositories, when we find fragments and blocks which consist entirely or in part of ores, or of minerals which accompany them in their beds or veins; and the last are often the most important, inasmuch as they probably indicate in what rocks the repositories occur from which the ores are derived. We may at present pass over the conclusions to be drawn from the sharp or blunt angles of the fragments, and the more or less complete rounding of the masses, for every thing upon this topic which is not in itself self evident, may be found in all books which treat of this subject; but it must be remarked, that although the fragments and masses give no indication of the occurrence of ores and useful minerals, we must not, from this cause alone, unconditionally assume their absence, or be deterred from prosecuting the search before it has actually commenced, for a number of causes may prevent the appearance of fragments or boulders of such minerals in the beds or on the banks of rivers, although repositories of importance and value may probably exist in the district.

Sometimes there occur in the valleys and on their declivities, also on the ridges of the mountain-ranges, fragments and blocks which are not derived from the district, but have been transported from remote places. The mode of distinguishing these from the others, and the judgment to be formed from their presence, may be ascertained from their composition, and from a comparison of their nature with that of the mineral masses occurring in the district under examination.

With the consideration of the fragments and the rolled

masses, we must also combine the examination of the sand which the rivers deposit in different portions of their vicinity on sand banks, &c. and we must examine it not merely at its surface, but as deep as it extends. The usual and well-known methods are to be employed, and although neither gold nor any other useful mineral be found, we sometimes obtain valuable indications for the prosecution of our researches.

We are now to proceed to the more minute consideration of the two sides of the valley. We determine, in the first mountain-mass which presents itself to our observation, the kind of rock, and its particular variety, and take specimens of it, in order to institute a comparison with others. The direction and dip are to be ascertained with the greatest possible accuracy, and also the extent, when the form and structure admit of it. We have to investigate the structure of the mountain-masses or that of distinct concretions, and we have to consider attentively the surfaces of the distinct concretions (*zusammensetzungs flächen*), to know if there are not other rock-separations distinct from the surfaces of the distinct concretions, and if these surfaces are separated by any intervening substance such as loam, clay, or of any other mineral or minerals. If there should be such separating substances, we ought to take specimens of as distinct a nature as can be obtained. We should also not leave unnoticed the minerals foreign to the composition or nature of the rock, and which occur either disseminated or in any other way distributed, and should examine the condition of the rock, as to whether it is perfectly fresh, or if it has suffered a more or less complete decomposition, either in whole or in part. The cause of such decomposition is to be carefully investigated. If we find that this change proceeds from a seam of the rock, or a fissure, which is not an ordinary surface of a distinct concretion, we are to follow this fissure so far as is permitted by the exposure of the rock, and to observe attentively what is presented by its continuation. This investigation is to be extended over the whole mountain-mass, so far as it is accessible on the declivity, and an opinion is to be formed regarding the portions which cannot be reached, by means of the fragments which have fallen down from them.

No exertion is to be spared, and we are to leave no phenomena, however trifling they may seem, unobserved; for whoever is acquainted with mountains to any extent, must be aware how important for our present object, or even for purely geological purposes, an apparently insignificant circumstance may become.*

After all this has been attended to, the superficial relations represented on the map, and the other circumstances carefully noted down, and after sufficient specimens have been collected, we proceed to the next mountain-mass, if there should be another present. If there be no other, that is, if the valley lies, throughout its whole extent, in one and the same mountain-mass, it happens not unfrequently that this mass presents several varieties of rock, and many differences in its structure on the large scale. All such are to be observed, and we proceed with them exactly as we have already described. Of course there must be lateral valleys or ravines on the two sides of the transverse valley with which we are occupied; and it is plain that we must extend our investigations to all their separate portions up to the summit itself of the mountain-range, and thus attend to the position of such small notches, as it is often less exactly laid down on the map than the position of the larger valleys. It is not superfluous, also, to mark in the enlarged copy of the map, the bendings, the enlargements, and the contractions of the valley.

If, in the valley under investigation, there should occur a series or alternation of several mountain-masses, the most important subjects for examination, after those already mentioned, are the boundaries between each two. These boundaries, or the surfaces of superposition, are generally somewhat difficult to discover, for they are seldom exposed, at least for any considerable extent. They are generally covered by fragments, or with earth or vegetation, or the rock is, in their vicinity, much destroyed and decomposed; and we are not un-

* Such investigations teach us more especially that many of the phenomena which we observe, particularly in structure and superposition, cannot be explained by general or partial, or by slow or rapid and sudden elevations, or, in short, by mechanical causes.

frequently first rendered aware by this circumstance of the change of rock. The fragments of the rocks on the two sides of the position indicate where we are likely to discover them. They are then to be sought for accordingly in the declivity. If we are not so fortunate as to meet with exposed points, we still can find out the places where the surface of superposition can be uncovered with least difficulty. We are to follow this mode of procedure at as many points as may seem necessary to convince us that we have become sufficiently acquainted with them, and ascertained their nature. The circumstances and relations to be attended to are chiefly the following:—*First*, The position of the structure of the rock masses on both sides, on the great and small scale, relatively to the surface of superposition; if it is parallel to it, or if one or the other intersects it; if it is bent; if it approaches the other, or if it undergoes any kind of derangement;* likewise the condition of the rocks in the neighbourhood: if they are unaltered, or if they are altered, and in what respect; if they contain minerals which were not previously found in them, &c.† *Secondly*,

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* In certain kinds of mountain-masses, a state of disorder is assumed to be almost universal, in respect to the structure of the slaty rocks with which they come in contact; although the observations made upon them seldom contain any thing of the kind, and indeed only in cases where the phenomena can be explained by structure alone.

† It is customary to attribute the presence of such minerals, for example garnet and some others, to the plutonic influence of certain granular mountain-masses, that are in contact with the slates in which these minerals are found (sometimes, however, also in the granular rocks). It is doubtful, however, if such an explanation can be applied to ores, pyrites, glances, and blendes, which are enumerated as occurring in such situations, and the appearance of these minerals, under such circumstances, may, of itself, contribute to diminish the probability which we might otherwise be persuaded to assign to this explanation, in so far as regards garnet, analcime, &c. On the other hand, the occurrence of these, especially when they are found in both rocks, belongs to the proofs of simultaneous formation, of which some have been already given in the previous part of this treatise. (Lyell, in his Principles of Geology, 3d edition, vol. iv. p. 286, says, "It is also an important circumstance, that, near the point of contact, both the granite and the secondary rocks become metalliferous, and contain nests and small veins of blende, galena, iron and copper pyrites.")

The surface of superposition itself: if it presents itself as a real rock-separation, that is, a true distinct concretion surface (*zusammensetzungs fläche*); or if the rocks of the masses on both sides are sharply and strongly united with each other; or if they pass into each other, and in what way they do so? *Thirdly*, If the appearance of another rock is combined with these passages? We know that, on the boundaries of two mountain-masses, conglomerates often occur, by means of which the passage takes from the one to the other; or, that, in place of these, layers of clay, &c. present themselves. But we also know that repositories of useful minerals often occur between two different rocky masses; and hence, although we may not find such minerals themselves, yet we must take care to ascertain if what we actually discover may not be connected in some way or other with repositories of the description mentioned. It is plain that, for this purpose, a very exact knowledge is requisite of these repositories, especially of their barren masses ("*ihrer tauben mittel*") and otherwise unproductive portions which generally remain unnoticed; and it is likewise evident that, when we believe ourselves in some degree justified in suspecting something, we must endeavour to ascertain as much, and to inform ourselves as far as is possible, without actually beginning the operations of a search. Here, also, we ought to notice the shoots of one mountain-mass into another, and also the frequent alternation of small beds, ere the succeeding mass appears in full size. *Fourthly*, The condition of freshness or weathering of the rocky masses on one or on both sides of the surface of superposition; and, lastly, *Fifthly*, The position and direction of the surface of superposition itself. This surface is rarely an even one, and indeed often deviates considerably from it. We must not, therefore, at least every where, take for granted the regular bed-like form of the mountain-masses, even though we should find them in a perfectly conformable position, that is, the structure of the rock and the mountain-masses (the last being what is termed stratification) parallel to the surface of superposition, but we must wait for further observations, and combine them properly with one another, in order at last to

be able to form a sufficiently accurate opinion of their form and other relations.

When the new mountain-mass has been examined in all the particulars which have been pointed out, we advance farther, and conduct our investigation of the next mountain-mass in the same manner. We proceed in this manner until we attain the high mountain-ridge, regarding whose relations we had already made ourselves previously acquainted. The other declivity of the valley is then to be investigated, probably from above downwards, and we may rest assured, when all this has been carefully observed, we have done every thing that could be done, so far as we have gone, in a search of this kind. The phenomena of stratification with which we have become acquainted in the first transverse valley, will lead us to decide if we should, in the mean while, pass over one or several of the neighbouring valleys, and direct our attention to a more remote one, which is probably distinguished from the others by its position and constitution, or if we should investigate them in the order of their sequence.

If we have found, in the valley that has been investigated, that the stratification on the two sides is extremely regular, and if we have seen that the structure of the mountain-masses on the large and the small scale, is parallel to the surfaces of superposition, and that it is as perfect a plane as can be expected in such circumstances, we may, with some reason, presume, that these relations suffer no essential change in adjoining valleys; and, as it is of importance to obtain a general knowledge of the whole phenomena of the stratification or of the constitution of the tract, it will probably be advisable to pass over the neighbouring valleys on both sides, in order to select valleys at a greater distance, from which we may expect to be able to draw new inferences. But these adjoining valleys ought not to be entirely placed aside, and they deserve to be examined with as much care as the others, for we never can know that they may not lead to discoveries which could not have been anticipated from what we had learnt in their vicinity. If, in the valley which has been first examined, the phenomena of stratification should be less regular, and if the individual mountain-masses should appear more in irregular

shapes (*stockartigen gestalten*) than in beds, being sometimes of a greater, sometimes of a smaller extent, and should not present relations corresponding to a normal stratification, the necessity of ascertaining the connection of the masses and the actual nature of the arrangement, would cause us to examine the adjoining valleys, and not only to do so with the care bestowed on the first valley, but to prosecute our investigations respecting the real nature of the phenomena, until no doubt should remain on the subject, and until we should be able to delineate them without fear of error on our map.

We thus proceed to investigate by degrees the valleys of the one side of a longitudinal valley throughout its whole extent, and then direct our attention to those of the opposite side. Should there be several longitudinal valleys on the same declivity of a mountain-range, which cross this declivity either in contrary or in more or less parallel directions, we proceed with these in the same manner, and examine those valleys which are transverse at their origin or near it, but which, in their progress, become longitudinal ones, and either remain so to their termination, or when near it, again become transverse ones. After completing fully the investigation of the one declivity of the mountainous district, we proceed to the other, in order to treat it in a similar manner; and we have thus our attention more especially directed to the differences which exist on these two declivities, and which, notwithstanding their proximity, are often much greater than might have been expected previous to the investigation. Rocky masses, which on the one side appear in moderate extent, occur on the other in much greater development; those which present themselves here over an extensive tract, and in great thickness, are found there in smaller quantity, or disappear altogether. Even the external form of the two declivities, and the constitution of the valleys, is not every where the same; and hence it follows that we cannot, from our knowledge of the one, draw our inferences respecting the other, and that both must be investigated with the requisite degree of care.

When this examination has been finished, the next step in importance is to combine the observations on the map, and especially those relating to the extent and boundaries of the

various rocks, &c. (*räumlichen verhältnisse*). We must carefully avoid going beyond what has been directly observed, and must therefore insert nothing which does not result from personal observation; for it is in this way alone that we can be made aware of what we have still to learn. When we combine remote points of observation by lines, without reference to the points lying between, erroneous views may be formed, which we ought to be on no occasion more anxious to avoid than in an occupation of this kind, where errors are, in the very nature of the case, so easily possible, and at the same time so productive of evil.

But when we have, with proper care, delineated on the map the observations which have been collected, we can then judge of the identity of the mountain-masses in the various valleys that have been examined, and of the beds which are distinguished by a different mineral constitution, and we can then obtain a complete knowledge of their extent and distribution. If we find that a mountain-mass, or a bed, does not make its appearance where, in the regular course, it might be expected, we must examine the causes, and we shall find them to consist in this, that the mountain-mass has become altered in the nature of the rock of which it is composed, or that it has wedged out. The one phenomenon is as important as the other; for with the alteration of the rock there are often on the surface of contact new phenomena, which may be advantageous or the contrary, but which must be, in either case, ascertained; and at the wedging out, other mountain masses come in contact with one another; and what farther takes place must be determined by direct observations, and must not be assumed from probabilities or suppositions. If another mountain-mass has presented itself, then all kinds of special occurrences come into consideration, and must be judged of and treated as has been previously described. If a mountain-range should possess a very considerable breadth, it is necessary, even though none of the circumstances just detailed exist, to examine it more closely, if we have not already made ourselves sufficiently acquainted with the range by means of the lateral valleys and ravines, in order that we may acquire as many data as possible for ascertaining perfectly the constitution of

the mountainous tract, and thus be capable of delineating the map and sections whose position and direction we can now, for the first time, determine, in the most proper and advantageous manner. For it is such maps alone, and such sections alone, which can be of real service, and their superiority consists in this, that they are founded on no theory, but on direct observations and investigations, and that they will always remain available, inasmuch as they will remain accurate in all time to come.

It will happen rarely that an investigation, conducted in this manner, should be completed without an indication being afforded, in one way or the other, of repositories of useful minerals. But should this be the case, it is a result which has its use, for it teaches us, not that there are no repositories of this kind in the tract, for the search does not expose its interior, but that they cannot be found out by such a procedure, which is the only judicious method that can be applied, and that their actual discovery must be left to the occurrence of accidental particular events, which experience teaches us has not unfrequently led to discoveries of every description.

As it would be unseasonable and precipitate to follow after every trace of a repository of ore, of whatever description it might be, at its first discovery, (except where the discovery were of such consequence that we might be satisfied at once of its value), so is it now time to follow up these traces and indications with all zeal and perseverance, as this can now be done with the requisite auxiliaries, and, therefore, with the hope of a favourable result. I do not mean that a discovery of this kind should be entirely overlooked during the prosecution of the search in the progress of which it has been made; but only that the course of investigation should not be interrupted, and that an unnecessary amount of time should not be bestowed on it, inasmuch as the most appropriate means of making the most advantageous use of it cannot be ascertained till a later period.

If the presence of useful minerals has been indicated by the sand of the banks or channels of the rivers, or by the fragments and boulders on the declivities of the hills, we shall at least be able to say where we may, with the probability of suc-

cess, look for such repositories; and if the repository has been discovered, we shall be able to determine in which direction we should direct our observations, in order to investigate its continuation and its exact constitution, as the original locality may not be adapted for conveying this information. The operations with which we are to proceed, in either case, are well known; and the circumstances will, after reflection, guide us better than general rules. The subject is treated of in several works, and more especially in the excellent Introduction to Mining, by Delius.* If the repository which we are examining be a bed—that is, if it be conformable to the structure of the mountain-masses and of the rock—its continuation is easily discovered; for, if we have carefully noted the nature of the rock, and accurately observed the direction of the structure, we have obtained a sufficient guide to it. If the covering on the surface does not admit of our following up the bed, we must uncover it from distance to distance. If in this operation we wish to avoid any unnecessary expenditure of money and time, we must be perfectly informed of the inclination of the repositories, and must combine this with the known relations of the surface, in order that we may be able to determine, from point to point, the line described by its outcrop. It is self evident that this cannot be accomplished with the necessary certainty without some measuring operations, and without some openings requisite for determining the amount of the dip; and, therefore, attention must be paid especially to the last, as they must afford data for the first. We are now to search for the repository at one of the points determined by the results of the previous investigation, and, when it has been found, to continue the labour in the same manner until we meet with it in a place to which our aim was particularly directed. If we should not have found the repository at the first or any other point, as is easily possible, owing to the variableness of its direction and dip, which can be known from the direction and dip of the neighbouring rock, when there is opportunity of observing its position and structure, we must still not give it up; but, if the constitution of the mountain-masses, in respect

* Section 1st, chapter ii., p. 163.

to their structure and other relations, gives no indication as to where it is to be found, we must seek to discover it by openings made down to the solid rock; the direction of which openings ought to be perpendicular to the direction of the structure of the mountain-masses or of the rock structure, or, where that cannot be recognised, to the known direction of the strike of the repository or bed. We take the portion thus ascertained as a new starting point, and again continue to prosecute our operations a before. If, notwithstanding all the means employed, the repository of useful minerals should not be discovered at any such point, and indeed no trace of it detected, we must not rest satisfied, and far less abandon the whole search; but we must return to the last known point, and from it endeavour to meet with it at a shorter distance off, perhaps about half-way between the two. Whether we should find it here or not, we shall at least be guided as to how we are further to proceed. The object of this examination is to ascertain the way and manner in which the repository loses itself; that is, if it wedges entirely out between the roof and floor, or if it, as it were, disappears in the rock by its component parts entering into the mass, being collected in it, and these at last passing into the mere rock, or if it is altered from its original position, or entirely cut off by displacement. We can from these circumstances, in some degree, form an opinion of what is to be expected from the continuation of the repository; and the last case is the one which promises least, though likewise there is but little to hope when the repository changes its nature, and is, as it were, changed into the mountain-mass. Still, we must not entirely depend on such being the case, but must anew investigate the repository where the nature of the surface, and the exposure of the rocks, permit; and, with the assistance of the information acquired of the relations of the stratification, and the nature of the rock, this has no particular difficulties, so soon as we are assured, in a general way, of the existence of such a repository, and are acquainted in some degree with the circumstances under which it presents itself; and it is only when it is not to be found at any of the points, and when no other trace of it can be discovered, that we can assume that it has entirely ceased *in this direction*, and terminate the inves-

tigation in order to renew it in the opposite direction, or on the opposite side of the mountain, perhaps with the same result. Perseverance like this is necessary, and no one will think my advice exaggerated, who has sufficiently studied in nature the repositories not only of useful minerals, but also of other minerals, which are sought for without exact investigations and mining contrivances. We unexpectedly find a recurrence of repositories of minerals, which we have observed at one point or in a district under certain circumstances, after miles of country have intervened, and after they have probably been forgotten, and we often detect their identity by means of trifles; for auxiliaries like those which are taken for granted in the search of useful minerals, are not easily obtained by the travelling geologist. We must, therefore, pursue the investigation, at all events within the boundaries pointed out by the nature and object of the search, until a perfectly well grounded result has been obtained, even though such should be unfavourable; and this result can only be attained in the manner which has been pointed out.

We have previously spoken of a place to which our aim must be more especially directed during the investigation of the repository. That place is the one most conveniently situated, and the most suitable, and which allows of the exposed repository being examined as to its being worthy of mining operations. Thus, it does not follow that a repository should be every where, or any where, of a description to warrant these operations; and the ascertaining generally whether the repository is of sufficient importance, requires a particular investigation and judgment, of which we shall speak, in what is to follow. But it also does not follow, at least unconditionally, that a repository worthy of being mined can be excavated with advantage at every point; and hence we have not only to distinguish both with sufficient exactness, but also to search for and uncover those points which, if the repository is sufficiently valuable at any part, are to be preferred in the latter point of view. We should endeavour to uncover the repository at such points, in order to submit them to more minute examination. The commencement of actual mining excavations requires a particular, viz. a purely mining knowledge, and in that respect does not belong to our present subject;

but the examination of the question as to whether the importance of a repository would warrant further proceedings, requires that I should devote a few remarks to it.

In order to carry such investigations into effect, we must select a situation where we can reach the repository by experimental trials (*versuchbauen*). Mere experimental trials can be made at points where we have no intention whatever of carrying on mining operations, for they only allow us to ascertain sufficiently the mining value of the repository. For if we are convinced of this, it is perhaps of little consequence if we can employ the already made trials in actual mining operations or not, and this undoubtedly requires a careful examination, so that we may not, by ill-judged economy, afterwards cause inconvenience and annoyance, which cannot be got rid of without great and continued labour and outlay. A position which is suited to the examination of the mining value of a repository must admit of the repository being traversed by levels and shafts. What is necessary for such purposes requires no further remark at present, except that, if local circumstances do not otherwise determine the point, we shall generally attain our object better by means of levels in the case of beds, and by shafts in the case of veins, inasmuch as the former are not so variable at different depths as the latter; and that all these trials must be made upon the repository itself, and not across the rocky mass in which it is contained, if very peculiar circumstances do not call for the contrary; for, by means of a bore in the repository, we become acquainted with it throughout its full extent, while, by cutting across the rock, we only ascertain its nature at one point, and are therefore obliged to inform ourselves respecting the remainder by other operations.

After we have met with a bed of such a nature that we may hope for productive operations, we must not neglect to search for other similar beds in the same district; for experience teaches us, that not unfrequently several rich repositories of a like nature occur at short distances from one another, and the investigation causes the least expenditure of time and money, when we are at the same moment engaged with similar objects. Whether the repository which has been discovered, is an irregular mass (*ein liegender Stock*) of very considerable or of smaller

size, it is advisable to become acquainted with its proportions, that is, its extent in different directions; for, on these, other circumstances being equal, depends the value of the discovery. Local circumstances will indicate how this can best be done. In every case, however, the vicinity of such a repository must be well examined; and, as this cannot be accomplished in the same manner as in the case of a bed, a few remarks on the subject may not be unnecessary. If the mountain-mass which includes the irregularly-shaped repository has not a slaty structure, and exhibits no tabular concretions *(plattenförmige zusammensetzung)*, as is in some instances the case in limestone, we can only take the general relations of stratification as a guide to lead us to others, when we are informed of the presence of such repositories by our having already discovered one of them. We therefore examine the mountain-mass by means of its natural, or, when necessary, by artificial exposures, we attend particularly to fragments, &c., and extend this investigation to a sufficient extent, as it is generally a whole region or a mass of large dimensions in the mountains in which such irregularly-shaped repositories occur, so that from the discovery of one we may anticipate the existence of several. If the rock possesses a slaty structure, or if it presents tabular concretions, we must observe carefully the relation of the repository to the structure. For if the direction of the structure is cut by the repository, and not altered by its presence; further, if transitions present themselves from the mass of the mineral or of the repository into the mountain-mass, little else can be done but what has been already pointed out; but if the direction is so altered that the tabular concretions on the large scale exhibit bendings, and the smaller structure, or that of the rock, as it were clings to the mass of the repository, the latter will probably become contracted at the opposite end, and perhaps, after having lost its size, a trace may still be perceived on the surface of the distinct concretions, which will serve as a guide; and though this should not be the case, yet we can become acquainted with the tabular masses between which the repository is contained, and can follow them up. But, when we have lost all traces, we must more particularly notice the bendings which may probably present themselves in the struc-

ture of the mountain-masses and of the rock, and especially if the mountain-mass is otherwise pretty free from them, we must allow ourselves to be guided by them.

An acute observer will easily make out the modes of procedure, which in some other kinds of repositories can be employed in their investigation, when their first traces have been discovered; and it only remains for me to add some observations in regard to veins, as the discovery and examination of this description of repositories are often combined with great difficulties.

The outgoing of veins is not unfrequently so indistinct, that we can hardly make out what it really is. It often consists only of decomposed vein-stones, without traces of ore, and is composed of masses of mere clay or loam; and sometimes appears as a mere cleft, or several clefts, between whose walls hardly any substance can be remarked. We can pretty easily distinguish an actual vein-cleft, which is the outgoing of a vein, from an ordinary surface of a distinct concretion, or from a smooth surface (*Blatt*); but the discrimination becomes more difficult when, as not unfrequently happens, a surface of a distinct concretion, or a smooth surface, is combined with it. In such a case we must attend particularly to the adjoining rock. If it is fresh, that is, in its normal condition, as is almost invariably the case with mere surfaces of distinct concretion and smooth surfaces (*Blättern*), the cleft, although different from surfaces of distinct concretions (*zusammensetzungs-flächen*), is nevertheless not a vein-cleft; if, on the contrary, at a greater or less distance from the cleft, it is altered in its constitution as to colour, hardness, compactness, &c. and if this alteration disappears the further we proceed from the cleft, so that we may conclude that it proceeds from the cleft, and is produced by it, then it is probably a vein-cleft. These are the most doubtful appearances. If we find vein-stones in the outgoing; if the latter is of some size, and if we meet with traces of pyrites or ore in it, we have greater security; and if the vein appears with the usual peculiarities of these repositories, we are still better off. Such cases require no further facts to enable us to form our judgment and our plan of operations.

In the doubtful cases we must employ every means calcu-

lated to lead us to a certain result. Hence it is advisable to follow the outgoing as far as possible (and it is a good symptom if it permits us to pursue it for several hundred fathoms), in order to enable us to observe it at a variety of places. If there are alterations of the mountain-masses, we must particularly endeavour to trace the outgoing into these different masses; for it has been often observed that veins in the different mountain-masses alter their nature so much, that, unless we were aware of the existing connection from observations made at different points, we should not be inclined to view them as the same repositories. It is therefore possible that a doubtful, that is an unpromising outgoing, may be of such a nature in other mountain-masses, that not only the uncertainty has disappeared, but that favourable prospects have actually taken its place. But we cannot trust implicitly to this consideration, for we know well that veins sometimes traverse various mountain-masses without undergoing the slightest change in their nature.

If, from these, and probably from other observations and phenomena which, from their diversity, cannot be all introduced here, but which can easily be determined at the locality, we have convinced ourselves that we have to do with a true outgoing of a vein, we endeavour to trace it by the usual means to a place where the repository can be examined in reference to its mining value. It has been remarked in another place, that in this investigation we ought especially to avail ourselves of the assistance of trial-shafts, and the reason of this is, that veins frequently extend downwards, for ten or twenty fathoms, or even more, without exhibiting traces of ore, or indications shewing that the vein is worthy of the miner's attention, while at greater depths it opens out and becomes valuable. However, it can be easily understood that the employment of shafts depends on the deviation from the perpendicular of the surface of the rock; for it is evident that even when this is considerable, galleries for exposing or trying the vein can lend useful assistance, and, in the selection of a point for investigation, we must take all this into consideration.

What has previously been said of the mode of procedure to be followed for the discovery of repositories of useful minerals

in respect to a whole series of rocky masses, is applicable to each individual portion, if we have sufficiently ascertained the relations of that portion to the whole. Without this knowledge we are deprived of a great part of the guidance which in such operations may render valuable assistance; and hence we would do well to exceed the prescribed limits on all sides as far as necessary to assure us that we have perfectly comprehended the most general, and therefore the most important phenomena. As this requires no special notice after what has been already said, it only remains for me to speak shortly of the procedure necessary with respect to the object under consideration in the secondary rocks.

The constitution of the rocks termed secondary in plains, is much simpler than that of the mountain-masses in the districts which have been already discussed; and hence also the mode to be followed in searching for valuable repositories of useful minerals is also more simple; and of these we have only to consider those occurring in beds, as veins are to be treated in the manner just described.

The first thing to be done, in reference to a bed-like mass which has been found, is to ascertain that nothing more is to be sought for in or under it. If we are acquainted with its extent and breadth, we can not only limit more narrowly the district in which we are to prosecute our researches, but can also obtain an idea of its form and constitution, for the various rocky masses occurring in such a district are generally uniformly stratified in respect to one another. Hence, if we have found the outgoing of a bed of useful minerals, or if we have grounds for supposing the presence of such a bed, we are already in possession of sufficient information to guide us in its further exposure. The different cases which can occur may be referred to the two following. The various rocky masses either so repose on one another that their strike for great distances is a straight line, and their dip everywhere towards one and the same point of the compass, and that under nearly the same angle; or their strike changes its direction as well also as the dip, and in such a manner that the whole rocky mass is inclined on several or on all sides towards a certain line, or a certain point in the interior, or, to express

it shortly, presents a trough or basin shaped stratification. Under such circumstances, it is not difficult to follow out the outgoing of a mineral repository, when it has been discovered; or to investigate the repository itself, if we have sufficient grounds for knowing of its existence; and as in this kind of formations the nature of the rock and the phenomena of stratification can with peculiar facility admit of our employing the borer, so is this the best means for discovering as well as for examining the value of repositories of useful minerals in the secondary strata.

If now, we know, in reference to the first of the above-mentioned cases, those rocky masses in and under which the repositories sought for are not to be expected, and if we are fully informed of their strike and dip, we draw at proper distances, of some hundred fathoms, straight lines perpendicular to the strike of their rocky masses, which extend as far as is necessary from their hanging side in the direction of the dip. This can really be done on the map, but in the field by means of poles. In those lines we take the points at which we mean to proceed with our bore-holes. The distance of these points from the hanging side of the fundamental rock (that namely in and under which our rearches are not to be prosecuted) is determined by the amount of the dip of the rocky masses, and from the nature of the apparatus which we employ; or also from the knowledge of the general constitution, and extent of the repository regarding which our investigations are carried on. When the dip is inconsiderable, and when we do not avoid making deep bores, the repositories probably possessing considerable extent, these distances may be more considerable; but when the dip is greater, and when, from any causes whatever, the bores are less deep, the repositories being probably of smaller extent, the distances must be less. In the choice of points for the bores, we must take into consideration the nature of the surface, and also other circumstances which can promote or hinder the operations, and we rather deviate a few fathoms from the above-mentioned line, in order to obtain advantages of this kind, or to avoid disadvantages, but only not so far as to injure or render uncertain the result of the labour. After we have selected a suitable and convenient

point, we make the first bore down to the fundamental rock. This fundamental rock must be reached with certainty, and, in order to be fully assured of it, it may be proper to pierce it for a few feet. The rocky mass in which the first bore has been made, if it has been penetrated throughout its whole thickness, will be considered as the fundamental rock in further trials, but only in so far, that, when the first bore is made in another and a superimposed mass, it must be again reached by it, and not with the understanding that in it (in some degree in another series) nothing more is to be expected or sought for. If this rocky mass superimposed on the subjacent one, or any other of the rocky masses under investigation, be so extensive, or its dip so considerable, that it cannot be completely traversed by one bore; we then divide it according to its levels and the general relations of its stratification, into several parts, and treat each of these subdivisions as an independent one distinct in its rock and different from the rest of the rocky mass. But, as the surfaces of superposition generally, or the actual clefts of stratification which exist, are not perfect plain surfaces, and as all kinds of bendings occur and may cause errors in the result; in order that we may proceed quite safely, we ought rather to do too much than too little, and make the bores pass one another for a distance of some feet or fathoms. We thus carry on our labour in so many parallel series of bores, and in so many separate bores in these series as we think necessary, until we reach the rocky mass which forms the general covering of all the others, or regarding which we know that nothing more is to be found in or over it, and can feel convinced that, if we pierce this superimposed mass likewise and find nothing, then the whole examined series of strata can contain no repositories of useful minerals. This is the general mode of procedure which, according to particular circumstances, may be modified, and not unfrequently greatly curtailed.

We have presupposed repositories which extend over the whole system of stratification. How we are to vary operations with less extended repositories, is of itself evident. It is requisite in such cases to carry the bores which we have

made in the upper rocky masses down to the lower ones, if we wish to be assured that no repositories of smaller extent have been passed over.

If, as is the case with the coal-formation, we know the rocky masses in which the useful minerals must occur, in so far as these are present, and above and under which we would seek in vain, it is plain that we only pierce these, and that we do so in the same manner as in reference to a very extensive bed.

If the stratification is trough or basin shaped, the only difference to be attended to is as to the direction of the lines in which the bores are made, for these must converge towards the middle line, or towards the centre. The shortening of those operations, which have been here described to their full extent, can be effected according to circumstances, and to the opinion we may form, since we select from the parallel or converging rows of bores one or several, which we consider sufficient for our object. We thus become acquainted with individual sections; and although these can afford no perfectly conclusive result, yet they are more to be trusted than separate sections in the primitive and transition classes of rocks, inasmuch as, with the exception of certain cases on the one side and the other, the arrangement of these is seldom so regular as that of the secondary class.

By the mode now described, we acquire information as to the presence or absence of repositories of useful minerals, but in the case of the former, we obtain no satisfactory data for judging of their value. In order to acquire this, and at the same time to become acquainted with the phenomena of the repository at a point suited to future undertakings, we look out for one point, or, according to circumstances, several points, in which we find the necessary requisites for mining operations, and the fewest drawbacks which might prove troublesome. At these points we investigate more closely the nature of the repository, not only in reference to its products, but also as to its extent in all directions, and its thickness, and we must do this by means of bores, which must penetrate down to the bottom. The smallest number that can be employed is three; for, by means of three points not lying in a straight line, the

position of a plane is determined, and we must make ourselves well acquainted with the strike and dip of the repository, and especially the latter, in order to ascertain its thickness (which is not shewn by the length of the bore in it), and to be enabled to form a correct opinion of other circumstances. We make these three bores at measured distances from one another, compare the result which they afford with the general relations of the stratification, and assure ourselves, if we find that it so far corresponds with them as may be expected in cases of this kind, by means of some bores placed in proper connection with them, that this correspondence has not been accidental. But if we should find that the position and direction do not correspond properly with the other relations of the stratification, by which, according to experience, we must take for granted that these repositories are also regulated, generally speaking, and as a whole, but that they deviate more or less considerably from them; then we have to do with bendings or slips, or other irregularities of stratification, or with dislocations and faults, caused by backs (*Rücken*), and intruding alternations, or in general by vein-like formations. It is well known how many difficulties are caused to mining operations by such appearances, and I need dwell upon them no longer in this place. We must endeavour, with all diligence, to become acquainted with the circumstances which give rise to them, and if we do not find easy and safe means of setting them aside and rendering them harmless, we ought rather to go out of their way. This is also to be done by bores, and in many cases it will be advisable not to spare these, as, notwithstanding all our foresight, often enough unexpected circumstances intervene which increase the difficulty of mining. A number of bores will also be found useful by making us acquainted with the substances worth mining, and those which are not worth it, and hence they are serviceable in a variety of ways. Nevertheless, in causing these bores to be made, especially where it is requisite to give them a considerable depth, and thereby to render them expensive, we must go to work with proper caution, and it requires much attention, and knowledge of the repositories, if we wish to obtain the conviction that we have bored at the proper place, and

that we have neither employed bores too much nor too little. The sinking of a shaft may, under certain circumstances, be much more advisable, and one or several shafts may subsequently be necessary at any rate.

On the Law of Storms, as communicated to the American Journal of Commerce. By W. C. Redfield, Esq. of New York.

The editors of the daily press in this city, having contributed frequently of late to the promulgation of Mr Espy's peculiar views in relation to storms, will doubtless as cheerfully give place to the following *facts*, relating to the severe storms of December 15. 1839, at Boston and other places. These facts, if considered geographically, and with strict reference to one point of time, are within the comprehension of every reader; and form, therefore, a valid and useful test for the theory or hypothesis of an inward or centripetal course of the wind in a storm, which is advocated by Mr Espy.

To avoid all complexity, we will confine our inquiries, on this occasion, to the afternoon of Sunday the 15th, *at or shortly before sunset.* The evidence within our reach on this point is as follows:—

"1. At *Nantucket*, the excellent meteorological journal published at that place, states the wind at 7 A. M. and at noon of the 15th at east, with rain; *at* 9 P. M. *SW. fair.* Compared with the next two accounts, the change to SW. would appear to have been in the afternoon, previous to sunset. The changes here being always found to make progress north-easterly.

"2. At *Barnstaple* on the southern extremity of Cape Cod Bay, sixty-six miles from Boston, in a nearly SE. direction, 'it blew hard from 9 A. M. to noon; after which, while the gale was most severe at Boston, the wind lulled to a moderate breeze, and shifted to S. and SW., continuing through the afternoon and night.' Another account states that the wind 'was south at sunset, rather mild, and stars visible in the evening.'

"3. At *New Bedford, Ms.* as appears by the meteorological journal of Mr Joseph Congdon, the wind also changed, about 3½ P. M., from ENE. to south,—at 9 P. M. wind S., moderate and cloudy.

"The above three places would appear, at sunset, on the 15th, to have

been within the central lull of the storm, which reached Boston at 7 P. M., and around which the wide spread annulus *of wind*, which forms the true gale, appears to have been blowing with its full power. The lighter winds within the central lull, in many cases, conform more or less to the course of the storm, which would cause the wind to be south-westerly. In what follows, therefore, no stress will be laid upon the direction of these lighter winds in the centre of the storm.

"4. At *Provincetown*, near the N. extremity of Cape Cod, the gale was most severe from 11 to 4 P. M., on Sunday; its direction, by collating the accounts, would appear to have been from E.SE. During the night following, the wind is stated to have been *moderate* and all round the compass. This was the central lull, as the storm was renewed on the following morning, as well as at the above-mentioned places.

"5. Captain Slemmer of the brig Columbus, in his detailed and seaman-like account, states, that on Sunday at 2 P. M. he made *Sandwich*, on the west side of Barnstaple or Cape Cod Bay, the wind blowing a hurricane from E.SE. The weather lighting up afterwards, he ran into Plymouth. The direction of wind at sunset, after the lull, is not given us.

"6. A published letter from *Gloucester*, north-eastern extremity of Massachusetts Bay, dated on Sunday night, says: 'We have experienced a most disastrous gale of wind here to-day from E.SE.,—the rain continues to pour in torrents, and the gale has not abated any.'

"7. At *Salem*, fifteen miles from Boston, according to the Salem Register, 'During the day, at intervals, the wind blew with tremendous force from the *eastward*, and the rain fell in torrents.'

"8. At *Newburyport*, thirty miles N.NE. from Boston, according to the Newburyport Herald, the storm commenced on Sunday morning, and 'from ten to twelve o'clock on Sunday night, the wind, which had shifted a point or two *more to the NE.*, blew a perfect hurricane.'

"9. At *Portsmouth, N. H.*, some sixty miles N.NE. of Boston, in the meteorological journal published at that place, we find the wind recorded during the day at *east*, with snow and rain. In a Portsmouth paper this storm is styled a 'heavy NE. gale.'

"10. At *Portland, Me.*, as appears from a valuable sketch just published by the keeper of the observatory, the wind at 11 A. M. on the 15th was 'east, with heavy rain; P. M. E. by S., *gale still continued;* in the evening, wind shifted to NE.' &c.

"11. At *Nashua, N. H.*, on the Merimac, as we are informed by the Nashua Telegraph, 'the storm of the 15th was from NE.' All our information leads to the conclusion that this comprises the close of that day.

"12. At *Boston*, the Atlas says, on Sunday at 2 P. M., it 'commenced raining with a violent gale from *the NE.*, which lasted till 7 P. M.' The Mercantile Journal says,—'the wind blew with great fury from the *eastward*, and in the evening for several hours it increased to a hurricane.' The Daily Times says,—it commenced raining about 2 P. M., 'with a

violent gale from *the E.NE.*, which lasted till 7 P. M. Another Boston account says,—in the afternoon the wind ' became strong from the *eastward.*' The Boston Watchman says, ' the wind was strong from the *eastward*, accompanied by moist snow and sleet, which gradually changed to a driving rain, and continued during the day.' In view of all this, we may safely infer, that the direction of wind at Boston immediately before sunset, differed not greatly from E.NE.

" 13. At *Providence, R. I.*, according to the observations of Professor Caswell, of Brown University, the wind on the 15th was ' brisk at NE., which continued till 2 P. M. The barometer continued to fall till 4 P. M., and remained stationary till near 7 P. M,, the wind still at NE., and cloudy.' The Professor adds, ' I am not particular to mark the *exact* point of compass, nor, indeed, have I any means of doing so.' It appears probable that this locality was in or near the border of the central lull, after 2 P. M.

" 14. At *Middletown, Ct.*, as I am informed by Professor Smith of the Wesleyan University, the gale set in at N.NE., and continued to snow and blow very hard during the 15th, the wind rather veered round to N., in which quarter the wind was very strong at the close of the day.

" 15. At *Hartford, Ct.*, according to the Daily Courant, this storm, during its continuance, was " accompanied by a strong wind from the north and the north-west." The latter was probably the driving wind of Sunday night and Monday.

" 16. At *Northampton, Mass.* as I am informed by the editor of the Northampton Courier, the wiud during the storm of the 15th was from NE. Mr Espy, I perceive, states it to have been north at this place, but on what authority he does not inform us. We will, however, take N.NE., the mean of the two statements.

" 17. At *Amherst, Mass.* according to the observations of Professor Snell, the wind in the latter part of the 15th was N. by W.,—" severe storm of wind and snow." His place of observation is on low ground a little SW. from the college, which stands on a hill. Perhaps this may have slightly affected its local direction.

" 18. At *Hanover*, on the Western border of New Hampshire, as I am informed by Professor Hubbart, of Dartmouth College, the wind veered on the 15th from NE. to N. The facts here are furnished from the meteorological journal of Professor Young ; and as the winds in this journal appear to be all referred to the eight principal points of the compass, as is common, there is room to infer that in the latter part of the day the wind was somewhat eastward of the true north : but this need not be insisted on.

" 19. At *Albany* and *Troy*, which places I visited on the 17th, the wind late on Sunday afternoon, according to my best information, was somewhat eastward of north.

" 20. At *Athens, Hudson*, and *Catskill*, from information on which I can place implicit reliance, the wind at this time was at north.

"21. At *Litchfield, Ct.*, according to the Litchfield Enquirer, the wind during the storm from Saturday night till Sunday night, was high from the north-east to north.

"22. Near *New Haven, Ct.*, off the light-house, Captain Woolsey, of the steamboat Providence, informs me that on Sunday afternoon till near sunset the wind was stong at N.NW.; but at 9 P. M. it had veered to NW. and was very heavy.

"23. At *New York*, as appears from my own observations, carefully taken, the wind during the 15th, and before sunset, had veered from 'N. by W., strong;'—to 'NW. by W.,' a 'hard gale;'—which continued in the evening.

"24. At *Cape May, N. J.*, as appears by the marine reports, the wind at this time was blowing a gale at NW.

"The storm of wind and snow was severe throughout New England, and its limits on the 15th extending greatly beyond the Hudson.

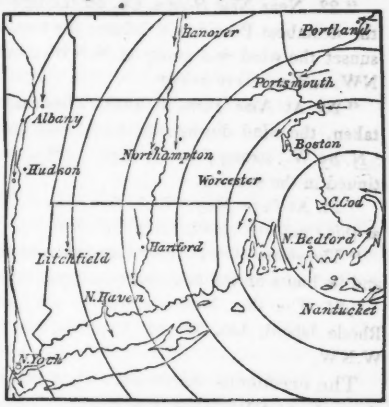
"25. The ship Morrison, on the 15th at sunset, 125 miles south of Rhode Island, Lat. 39° 35′, Long. 71° 38′, had the gale violent from W.NW.

The erroneous statement, that the steamer Providence had the gale at SW. near Newhaven on the 15th, which Mr Espy has used in at least one of the New York papers in support of his theory, he has since found occasion to correct: But I do not perceive that his correction has been given to the New York public.

I have brought together below, the chief of the foregoing observations, on a small map; in order that it may now be seen at a glance, whether the wind in this gale blew *inward* from all sides, towards the centre of the storm, according to the favourite hypothesis of Mr Espy; or whether, on the contrary, it was actually blowing *in a great circuit*, around its central portion, in the direction which is contrary to the hands of a watch which lies with its face upwards: as is found to be the case in all gales which I have examined; not excepting even those upon which Mr Espy is accustomed to rely, in his attempts to sustain his centripetal theory.

The circular lines on the map are drawn from a centre located on the southern border of Cape Cod Bay; and whether, or not, this is to be considered as the exact place of the axis, about sunset on the 15th, and the arrows near the several places as representing the exact course of the wind, is unimportant. These will at least serve as approximations; and the general result could not be materially affected by greater ac-

curacy of delineation, were the latter attainable.—To me, it clearly appears, that the wind was *not* blowing, at any time on the 15th, "towards a space or region of country south-west from Boston and north-east of New Haven," as was suggested by Mr Espy in the Courier and Enquirer, and the New York Gazette; nor, indeed, towards *any other* central space in the gale. The general course or manner in which this gale *did* blow about sunset on the 15th, when its centre was perhaps nearest, appears, I think, sufficiently obvious, from the foregoing statement.



The limited object of the present inquiry restrains me from examining the consecutive changes and relations of this storm, at the various localities; and much information, for which I am indebted to my correspondents, must for the present be passed over. It should be borne in mind, that our map comprises but a portion of the area which was covered at one time by this gale; which was greatly extended on all sides, but mostly on the east and south. It should be noticed, also, that the point of time to which all the observations are referred, approximates, as already intimated, to the time of the greatest fall of the barometer in this storm, in the states of Connecticut, Rhode Island, and Massachusetts.

The whole series of gales which have occurred, weekly, since the 24th of November, are well worthy of the attention of meteorologists; and the rotative character of each has appeared to be developed as clearly as in the case now before us.

Notes explanatory of some Passages in Professor Keilhau's Paper on Granite, &c., published in Vols. xxiv. *and* xxv. *of this Journal.*

Having been favoured by our esteemed correspondent, Professor Keilhau of Christiania, with some remarks on the translation from the Danish of his paper on the Theory of Granite, &c. which appeared in this Journal, vol. xxiv. p. 387, and vol. xxv. p. 80 and p. 263, we subjoin a few alterations and explanations, which we think will render the reasonings more clear and exact. It will, of course, be necessary for our readers to refer to the volumes just quoted.

In regard to the style employed in the above-mentioned memoir, it must be borne in mind, that it formed a part of the author's course of lectures delivered in Christiania. It must also be clearly understood, that in its first or introductory portion, that is, from page 388 (vol. xxiv.) to p. 399, the author, as he himself expresses it, follows "the mode of description adopted by modern geologists, or that of the volcanists," and adopts their language.

At page 389, line 20, the author intends to say, that the thin pellicle or crust of black slate is observed on the surface of the gneiss at *a considerable distance from the abrupt terminations of the strata of the slate;* and at the end of the same paragraph it ought to be mentioned, that the perpendicularity of the strata is particularly remarkable in the Christiania district. At p. 393, l. 6, the meaning is, that the porphyritic masses often occur divided into *several beds*, and that by pretty horizontal joints. P. 394, l. 17, read, "and sometimes that of veins, but always with the dimensions of ordinary beds and veins;" and l. 21, "*traverses*, for the most part, the stratified masses." P. 399, l. 29, &c. "Here we have seen that extraordinary disturbances in this respect have taken place. Although the beds are not very highly inclined in the vicinity of the great porphyritic districts, where, viewing the matter as Vulcanists would, the melted masses must have flowed out *horizontally over* the strata, yet we nevertheless remark, as a general rule, an elevation of the beds near the granitic districts, the masses of which have, on the whole, a more or less *vertical* position next the strata, which are supposed to have been elevated by this cause. . . . . (P. 400, l. 5.) It is true that a high dip is a common phenomenon in the district; it is also true that we only find exceptions to this rule in localities where the strata are covered by porphyries; so that at all other points (where, in fact, we are always near granitic masses) we generally observe an inclination more or less considerable. . . . . If we glance over the geological map, and attend to the circum-

stance, that the outlines of the granitic masses present extremely irregular sinuosities, and observe also that these masses lie, &c. . . . . It may be taken for granted, that the supporters of that theory, after having made their observations in about twenty places, and found nothing in the least corresponding with their ideas, would give up this labour, and that then, as I supposed in my description already given, they will remain satisfied with this brief conclusion, that the strata *have been deranged.* . . . . Three of my former pupils have faithfully assisted me in this extensive labour; and we traversed, two and two together, the districts where the massive formations have not taken the place of the stratified rocks. We followed a strict method, almost as if we had been performing the duties of a mining-engineer," &c. P. 401, l. 23. It could not therefore be, unless by an accident difficult to admit, that these portions of a whole (and, as well as the eruptionists, we assume this original continuity) should present such a conformable position of strata, if that had not existed before." P. 402, l. 1, the word " previous" should be left out. Vol. xxv. p. 82, l. 26. " The relations of the granite, and of the slate, are therefore not always such as we have previously described them; it is certainly more usual to find these rocks including each other at their junction, but this relation is not exclusive, and it ought to be added, that we observe, at least in some places, *perfect passages* from the granitic rocks to the siliceous slates." P. 83, l. 8. " Thus, in the case where they would adopt for the crystalline slates, which touch the granite by means of a sharply marked boundary, the mode of formation of which we have spoken, it would not at all follow that this explanation can be applied to the case where there exist perfect passages, so that it could not be said, for example, in respect to the Sölvsberg, that one portion of the granite existing there is directly pyrogenetic, while the other, serving as a passage to the slate, is rendered indirectly so by the influence of the first on the contiguous parts of the strata. If, however, any one, in order to support a particular theory, should give such an explanation of the parts of the Sölvsberg, he ought, so as to give some reason for these complete passages, to pretend that there exists here a special case, viz. that at the point where the two granites come in contact, they resemble each other to such an extent, that it is impossible to discover the slightest difference between them, and that the line of demarcation has become hardly perceptible by reason of the fusion, if, indeed, it has not been completely effaced." P. 84, l. 31, for *state* read *slate.* P. 84, at the bottom. " It appears that some of the supporters of this doctrine have endeavoured to explain the ramifications of the massive rock in the bounding strata, and generally the relations of the boundaries and positions of these rocks —relations which have of late been much more carefully examined— by assuming a simultaneous formation of the massive rocks and the stratified rocks which are often arranged together in so complicated a manner. But after what has been said, this mode of escape will not suffice; if it is maintained that the granite of the transition territory

was formed by crystallization at the same time with the stratified rocks, it might perhaps be pretended, with respect to the ramifications in the primary rock, that these were upfillings of the vein-fissures then open in the basis of the new formation." P. 86, l. 30. "It is not a mere conjecture, but a fact, *that the presence of the great porphyry formation depends entirely on this sandstone*, and, indeed, there is a constant relation observed between the existence of this sandstone and the points where the great porphyry formation has appeared. . . . . P. 87, l. 10. "When we treated of the coal-formation (in a previous part of the lectures), we hesitated to assert, in opposition to the authority of modern geologists, a constant connection between the existence of these porphyries and certain beds which are connected with the coal-formation; and we did not venture to admit a connection of formation between the abnormal mass and the strata in question." P. 88, l. 24, "That the larger masses are more crystalline than the smaller, accords perfectly well with vulcanism; but certainly this is not the case with the other circumstances I have just mentioned; it undoubtedly results from them, that these remarkable abnormal rocks which present themselves under the form of quartzites, syenites, or true eurite-porphyries, cannot in any manner be considered as a mere accessory and altogether foreign part of our formation, which might appear equally well in any other place, and even beyond the limits of that formation, but that, on the contrary, we should regard them as belonging to it in a peculiar manner; an opinion which prevents us from admitting the idea that they only occupy by accident the places where we find them." P. 92, l. 14. "In following up this subject, the first thing that presents itself to us, is the well-known phenomenon of the crystallization of substances which occur in the form of vapour or gas; but it is beyond this only that a dark region lies before us, into which, however, we must also penetrate." P. 94, l. 14. "It is quite evident that we must anticipate chemistry when we are forced to rest on facts like these quoted, but we could not expect that that science should condescend to give to the study of these phenomena all the attention which they require; in order, therefore, to render them available, we must take them as they are. The little that they tell us regarding the qualities of bodies, and the operations by which, in nature, the production of crystals and of mineral species is effected, is in fact the utmost that we can find where a basis is to be sought for our new theory, if, with this object in view, we do not wish to go beyond direct knowledge." (From the inquiry proposed at p. 92, as to whether there does not exist other modes than those admitted by the Vulcanists and Neptunists by which crystals are produced, it has resulted that there are two others: 1. solidification in crystals of substances in the state of an elastic fluid; 2. crystallization *of* solid masses and *in* solid masses, &c., *i. e.* by that method regarding which we yet know so very little. For the sake of clearness, this little *resumé* should have been inserted in the text, for, without it, there may be difficulty in perceiving that what follows is ar-

ranged according to that division of the two modes of formation of crystals, &c.) " We ought first of all to inquire, if the fact that substances in the state of an elastic fluid can be solidified in crystals, could be capable of application here; but even the first glance shews us," &c. P. 95, last line. " The dolomite theory supposes that the dolomite has been produced by the penetration of magnesia exhalations into masses of carbonate of lime. We ought, according to what we have said, to regard this hypothesis as a very legitimate one, although it goes far beyond what is taught us at present by chemistry. But what arrests us in this theory is, that, in order to support itself, it requires the assistance of other hypotheses of a very arbitrary description. It cannot be denied that in some places dolomite occurs in very thin but extensive strata, and quite in a regular manner between other beds; now, we cannot see why the magnesia should go and lodge itself precisely in those beds which are to be transformed, while it passes by, without producing change, those which preceded them in the order of the strata." (The dolomite is often found contained in carbonate of lime, and there are therefore unaltered beds beneath those which are converted into dolomite.) P. 96, l. 31; " Thus, if we suppose that these substances introduced into the rocks came from below, we might say that the great porphyry formation in our transition district is to be ascribed to the circumstance of the emanations having been lodged in the sandstone part of the series, while, on the other hand, the substances necessary for the formation of the eurite-porphyry had not been elevated to that height, but had united themselves to certain strata in the vicinity of the primary rocks, and so forth. But, in considering the subject more closely, we may leave out of consideration all these artificial speculations, and do not require to think of emanations, whether with reference to dolomite or to crystalline rocks. We are undoubtedly placed in obscurity here, and an obscurity which the lights of chemistry will probably not clear up for a long time; it is therefore of little use to trouble ourselves with hypotheses, in which, no doubt, homage is rendered to chemistry, but which can only be half acknowledged by chemistry."—P. 97, l. 15; " We are undoubtedly in possession of the following two most important facts: First, that masses in a solid state can crystallize, or undergo morphological changes, without its being necessary, so far as we can see, that they should previously pass into *one of the states of fluidity* with which we are acquainted; and next, that substantially newly-formed bodies, crystallized or uncrystallized, can be developed in compact masses likewise, without our remarking that the matter, immediately before the formation of the new body, has been in one of the states of fluidity known to us. . . . . For my own part, ever since I directed my attention to these, though obscure, yet important facts, I have always attributed an increasing value to them. Even in 1828, I endeavoured to bring prominently forward, and to prove this position, viz. *that one of the hitherto known forms of aggregation in which substances exist in a fluid state, does not necessarily precede the formation or transformation of solid bodies; but that, on the contrary, the compact form*

does not prevent the movement or mobility of substances. I said, it is then possible that in the solidified portions of the globe very essential changes may have taken place, and still take place every day, by means of this property of motion which substances possess in their rigid masses. And I added, ought we not *in this case* to conclude in favour of the reality from the possibility? Certainly, in modern geology, much less value is attached than formerly to the unfounded supposition," &c. . . . .

P. 98, l. 26; " A continued study has only confirmed my views on this subject, and I was constantly brought back to the application of this truth, which seems to me fundamental in the science, not only by the attentive examination of the mineral masses which form rocks, but also by the consideration of the productions which occur in particular repositories, in veins, beds, and nodules, and also by that of imbedded and disseminated minerals."—P. 99, l. 4; " We shall therefore endeavour to avail ourselves, as much as possible, of the observations mentioned. When we simply consider the facts on which they are founded, that is without attempting to penetrate to their very foundations, but regarding them only as mere phenomena, these facts lead to some general views which ought, at least provisionally, to be admitted as just;" l. 17, for *metastomatoses* read *metasomatoses*. P. 100, l. 31. " It is upon the same views, and in general from those which result from observations on the developments which take place in solid bodies, that we must, I believe, found our ideas of the formation of granite and the other massive rocks of which we are treating. We must return to the same source when we shall afterwards discuss the origin of gneiss, mica-slate, or generally of the *crystalline slates;* nay, the same leading idea will afterwards be made to apply to clay-slate, and most other *non-crystalline rocks;* for, when the subject is well considered, it is certain that these last themselves, regarding whose mode of formation so little trouble has been taken, have not been deposited just as we find them at present. But let us return to the unstratified crystalline rocks, which should occupy us at present, and particularly to those which we have observed in our transition series (*formation intermediaire*). In applying to them the results of the considerations which we have been considering, it will be well for us to attend at the same time to some other products of the same formation, whose formation is connected with that of the massive rocks." Vol. xxv. p. 267, l. 32, " In fact, the process which must take place, when a mass in a state of fusion becomes solidified, has not been minutely considered (for example, when such a mass is about to become granite). If it were to be attended to a little less superficially, it would probably be found, that the investigator would be led to embrace views but little different from those to which the preceding reflections have brought us, and the means which nature employs for the transformation *of an amorphous and homogeneous mass, which presents itself after that transformation, under a crystalline and heterogeneous aspect*, would be considered as identical, whether the mass may be what we would term solid, or pasty, or entirely liquid. Probably it is not too rash to suppose, that it will be

found that the essential distinction in the processes consists entirely *in the time* required," &c. In reference to the remark on alum-slate, at p. 271, l. 8, the author adds the following note:—" I would remark that there are two indications of the force which seems to react on the metamorphoses, and which may be said to impede them; 1*st*, That we do not find the ordinary changes near the granite when the primitive rock is very near at hand; and, 2*d*, That they are likewise not found at the contact of the eurite-porphyry—(because?) in both cases, it is the bituminous ampelite which touches the abnormal rocks." P. 271, l. 13, " That the porphyries, in general, *do not produce* those contact phenomena which are so distinctly displayed near the limits of the granite and the primitive rock, is a fact which deserves to be again mentioned. Masses in a state of fusion, whether, when they were solidified, they became porpyhry or granite, ought to have produced nearly the same actions."

Remarks on Professor Keilhau's Theory of Granite, &c. By Baron Jacob Berzelius.*

Keilhau, who has enriched geology with so many beautiful and important observations upon the subject of the relations of the junctions of the oldest rocks, and the various older and younger rocks reposing on them, and produced at different later periods, has, from the collected experience derived from these relations, which are often of the most singular description, and of a kind to defy our explanations, endeavoured to form a theory regarding their origin, which he made public in the 1st vol. of the New Magazine of Natural History, under the title of " The Theory of Granite and the other Massive Rocks," &c.†

The Norwegian Geological School of Esmarck, its much esteemed founder, who directly derived his knowledge from the chair of Werner at Freyberg, originally held the opinion of Werner as to the formation of rocks, partly by crystallization, and partly by precipitation from water. In this doctrine Keilhau was educated, but his own observations have gradu-

* Translated from his " Arsberättelse om Framstegen i Fysik och Kemi," for 1837.

† See this Journal, vols. xxiv. and xxv., and the present number, p. 350.

ally convinced him of its evident errors, and it may be now considered that he has fully forsaken it. In its stead, the opinion has begun to gain the general sanction of geologists, that the oldest rocks proceeded from the cooling of a melted mass, that this cooled mass, during the first period of cooling and hardening, was burst asunder and contracted, that new melted masses were pressed out from the rents thus produced, and that these again were hardened above the others. It is considered, and certainly upon good grounds, that these cooled masses having been ejected, and therefore altered in their position, were at first very hot; that water, for the most part in a gaseous form, surrounded the globe, and formed part of the earth's atmosphere, which became condensed incessantly, and fell back upon the heated surface, where it again got into a boiling state under an enormous pressure of its own gas, and therefore reassumed a gaseous form at a much higher temperature than that at which water boils at present; and that under this constant dropping of water in heavy rain, whose temperature was very high, the cooling surface was broken up, and became disintegrated and destroyed, in consequence of which the rocky dust was formed, which afterwards gave rise to stratified rocks, &c. These likewise were burst asunder with the underlying solid rocks, so that new melted masses broke forth in the openings of the still heated but hardening mass, where they remained for a long time liquid, penetrated into all fissures and openings, separated loosely united beds, flowed in between them, and, owing to their inferior specific gravity, elevated them: while, during the long continued hot condition caused by the extremely slow cooling, they altered the rocks with which they were in contact, and extended this effect to various distances, according to the different periods which the melted mass required for cooling. In this manner, it is supposed, that the transition limestone, which has evidently been hardened in water, was changed into marble, a condition which is closely allied to the mountain-limestone, from which it is principally distinguished by the fineness of its grains. In this manner also we see how, in still later periods of the cooling of the earth, the trap burned into hardness the upper surface of the bitumin-

ous shale over when it came out, and which, by the burning of the carbon, was changed from black to red and light gray. But this representation embraces only a small part of the causes operating during these events, it gives no information as to the origin of the transition-rocks, and still less of the deposits above these, which bear evidence of changes, whose history is lost, and cannot be restored by any theoretical speculation; for the occurrence of circumstances of which we have not been witnesses, or which have not been represented to us by eye-witnesses, must be lost to us. The last mentioned remain, in the mean time, altogether beyond our present subject, for we have only to do with the relations which present themselves in the contact of the oldest rocks with those which were formed at immediately succeeding epochs.

Scarcely any country possesses so many remarkable, and, at the same time, accessible phenomena of these contact-surfaces as Norway, and certainly no geologist has so carefully examined them as Keilhau. After he found that they are, in the most direct manner, opposed to the Neptunian views, or the Wernerian theory of the formation of rocks, he endeavoured to explain them according to the preceding shortly described Plutonian system, but he found this system so beset with stumbling-blocks, that he found himself obliged to view even this theory with doubt, and to seek to explain the phenomena in another manner. After having considered various of the chief phenomena which seem to be irreconcilable with the Plutonian system, particularly as to the Plutonian infiltrations of the rocks referred to, which are found in strata alternating with such as are evidently not Plutonian, where the strata sometimes become quite thin, and exhibit many alternations over each other; also the fact of the conversion of the transition-limestone containing petrifactions into marble, being continued to a greater distance from the pyrogenetic rocks, than the assumed hot condition could perhaps have extended the influence of its high temperature, with many other circumstances, which our space here will not admit of our stating, he comes to the theoretical view by which he considers these phenomena can be better accounted for. The description thereof is preceded by these true, and perhaps unchangeably correct remarks, "that the moment is not yet

arrived when a full description can be given of the phenomena examined, and that it is therefore most natural we should confine ourselves to the use of the facts which may enable us to form the first steps to an elucidation, such as the facts may be, without going into their remote causes."*

This first step towards the elucidation, Keilhau thinks can be made by means of the known power of matter, by which substances of a similar nature are attracted to each other, somewhat in the same manner as in the roasting of some of the inferior copper-ores, the metal collects at certain points in the roasted mass; and as some formless masses gradually obey the power of crystallization, so that their parts approach and give rise to crystals. In this manner he hopes to be able to employ chemistry in explaining what occurred at the formation of the rocks, and thinks that, inasmuch as this science is far from having reached the limits to which it may be carried, it must be permitted to anticipate this extension of the science by theory, while, however, care must be taken that speculation should assume nothing but what will probably be afterwards confirmed by chemistry. Regarding these above-mentioned collecting and crystallizing powers, as principal agents in the formation of the crystallized minerals and rocks, Keilhau has sought long since to establish the following doctrine: (Berzelius quotes the passage, which will be found at p. 353 of this number, "that one of the hitherto known forms," &c.) I shall now, in his own words, give his account of the rising of granite into the transition-formation.† * * * *

Keilhau supposes, moreover, that a similar crystallizing power, at the locality where the granite-forming process now spoken of was developed, began to change transition-limestone into marble, and his opinion as to the formation of gneiss and mica-slate is, that these rocks were produced by peaceful transforming processes, altogether analogical to that which produced granite and the other crystallized rocks, in which the slaty or stratified structure was lost in the original materials.

But Keilhau has not concealed from himself the difficulties which present themselves in following out this theory; one of

* See this Journal, vol. xxv. p. 97.

† See this Journal, vol. xxv. p. 263 to the bottom.

these is, that clay-slate does not contain all the materials of which granite and syenite are formed, and sandstone still less those of porphyry, and the other materials, moreover, are not in such proportions as to enable us, in a chemical point of view, easily to comprehend the possibility of the alleged processes of change. But this difficulty he has endeavoured to overcome by the before-mentioned proposition, that, if the knowledge we derive from chemistry does not suffice, we may be permitted to go beyond it. It has been supposed, that the simple elementary bodies are not so numerous in nature as we are at present obliged to believe; it may, therefore, be supposed, that the small number of actual elements can be changed in different circumstances so as to form different substances, and Keilhau considers it more reasonable to suppose such a conversion than that any new material has been introduced into those regions for the purpose of operating change. Another difficulty connected with the circumstances, is the beds of greenstone in which are found detached pieces of gneiss, and, in general, pieces of the rocks forming the walls of the veins. These agree with the idea that a melted mass was ejected from beneath, into which pieces of the surrounding rocks had fallen, and been enclosed by the cooling mass. "I willingly admit," says Keilhau, "that the opposing theoretic difficulties are considerable here, but if it should be entirely impossible to overcome them in certain cases, still I find in them no sufficient ground for giving up the views entertained, and returning to those (namely, the Plutonian) which, not merely in one respect, but in many respects, prove to be erroneous and unreasonable."* He contends, moreover, that these opinions are not intended to exclude others, and, if it were possible, that they ought to be placed side by side with the Plutonian theory of formations. Keilhau concludes with a view borrowed from one of the philosophical systems of our times.†

I have now, in as compressed a form as possible, stated Keilhau's theory, and should not have exercised my calling as referee, if Keilhau had not placed so much confidence in my

* See vol. xxv. p. 266 and 267.

† See vol. xxv. p. 271 and 272.

opinion as chemist, as to call on me, to state, in this Report, how I view, in a chemical point of view, the elucidations he has attempted.

With expressions of unfeigned esteem for this meritorious geologist, I must at same time regret that I cannot concur in his views of the subject. The power he refers to as the operating cause certainly exists, and, as such, must assist, like the other powers in matter, in geological operations, but it plays there, according to all appearance, a very inferior *role*, or it has a very small energy, and the circumstances under which it operates are quite circumscribed. These circumstances consist in a liquid, or at least a soft state in the masses, arising either from the power of moisture or of heat. There are certainly cases where it operates in bodies of a low degree of solidity, that is where the force of cohesion is weak, for example, when the yellow crystals of ioduret of mercury after being slightly scratched become again red and undergo a change in the relative positions of the particles; and when certain crystals pass in their interior from one fundamental form to another; but in this, however, there is no transference of particles from one place to another; the external contours remain, and the changed crystals, which have undergone any internal change, retain their edges and angles equally as sharp as before. The operations are thus confined to the nearest contact, and are accompanied by no absolute change of place in the solid bodies. No circumstances occur to prove that this power can work in another way in geological phenomena, namely, at a greater difference of distance, or make a greater resistance, than in chemical operations of a small extent. Glass which is kept soft in a heated state for some few hours, changes to Reaumur's porcelain, that is, the particles are subject to the power of crystallization to a certain but imperfect degree, but the glass in the Roman cups and bottles, preserved to our time, and in the glass pearls from the mummies of Thebes, are, after being preserved for thousands of years, still glass. If, according to this hypothesis, clay-slate, by the process of granitification, is formed into granite and syenite, and sandstone, by the same chemical powers, into porphyry, while the component parts,

in the original mass, are not the same in quantity and quality as in the products, this, in my opinion, is a decisive proof against the correctness of the hypothesis. It is a mistake to suppose that it is possible to substitute speculation for the natural relations which science has not explained, and the mistake becomes more important when such speculation supposes something which is in opposition to what is taught by the fundamental doctrines of science. Each step beyond the certain boundaries of science is nothing else than a fiction which is usually proved to be incorrect by scientific investigation. In the treatment of such subjects of an investigation as cannot be explained in the present state of science, a greater degree of scientific acuteness of perception consists in perfectly understanding what cannot be explained, than in anticipating science by suppositions. We cannot advance farther than to comprehend what can be understood, and clearly to be aware of what cannot be understood.

In regard to the greenstone masses with the included pieces of gneiss, this can only be explained under the so-called Plutonian theory, for which the presence of our existing volcanos, the universal regularly increasing heat of the earth towards its interior, as well as the still continued shrinking of the crust of the earth, that is, the gradual elevation of some regions and the sinking of others, afford so many remarkable proofs. Besides, this new attempt at explaining is not requisite, even although it did not bring forth scientific proofs of its incorrectness; for if greenstone had been pushed up from below in a liquid state, granite, syenite, and porphyry must have been so likewise. Supposing that the latter present phenomena, in the superimposed rocks of a different origin, which seem to defy a mechanical explanation; still this impossibility, in examining the mechanical details, does not carry with it any proof against the fundamental idea, that they came from below in a liquid state. Who can take it upon himself to explain the formation of the highly fantastic forms of the snowfields? asks Sefström, in his admirable examination of the origin of boulders. That snowfalls and storms are the principal agents we all know. The circumstances which operate in the

shaping of the wreath escape us, but this is no reason why we should doubt the existence of snowfalls and tempests?*

* In regard to the discussions connected with Professor Keilhau's geological opinions, we subjoin the following extract from a letter which we had the pleasure of receiving from him.—EDIT. "My fundamental idea is, that there exist *geological necessities* which must be respected, even though they should be opposed to some theorem of chemistry,—a science which, like geology, is a progressive one,—and that at present it ought to be our especial object to make ourselves fundamentally acquainted with those necessities, those clear facts which admit of only one interpretation, and which lead directly to axioms, just as well as chemical experiments. In the report of M. Berzelius to the Swedish Academy, where he has discussed my memoir, the conclusion he draws is simply this: By chemical analysis we do not find the same component parts in granite as in argillaceous slate; it is *therefore impossible* that the first can be a metamorphosis of the other. Therefore the geological facts are here of no consequence. In my opinion, on the contrary, the geological facts are in some cases so positive as to the transformation of the slate into granite, &c. that absolute confidence cannot be accorded to chemical analyses, or rather to chemical ideas deduced from them, which do not agree with the geological idea of transformations. In geology and in chemistry, we endeavour to observe facts; if this object were possible in the greatest degree of perfection, the two sciences would always be in harmony; but observations and researches being always more or less imperfect, there result discrepancies; and here I maintain, that it ought not to be considered that it is the geologist in every case who has observed the most imperfectly; I believe that there are phenomena, at the solution of which the geologist can arrive before the chemist, by following the path of pure experience. It is nevertheless true that such cases have hitherto been rare, and that in geology, the practice has rather been to advance at full gallop on the path of hypothesis, than to search carefully after conclusive facts.

"In the report just mentioned, our distinguished chemist grants the existence of chemical actions in solid bodies, but he does not believe that the force operating in them has performed so important a part as I must suppose, in admitting the successive granitification of slates, &c. But of that it is for geologists to judge, and they should not forget the views of modern physics respecting a perpetual molecular movement of a mass apparently in repose. Nevertheless, geologists themselves are perhaps at present but little in a condition to discuss fairly this question; for they seem to me too much carried away by the 'doctrine' of the internal heat of the globe. The following is a striking circumstance of the present time. Geologists who are Vulcanists, require the decision, that the earth is in a state of fusion at a short distance from the surface; and natural philosophers of distinction have not hesitated to pronounce such a decision, merely to partake in the honour of having constructed the definitive theory of unstratified formations, of chains of mountains, of volcanos, of earthquakes, &c. For myself, at least, I see no other motive; for if it were not desired to secure part of this glory,

Presentation of the Wollaston Medal for 1840 *to* M. Dumont, *and one year's proceeds of the Wollaston Fund to* Mr James de C. Sowerby, *by the Geological Society.*

Dr Buckland, President, on presenting the Medal to Dr Fitton, to transmit to M. Dumont, said—

"Dr Fitton,—I am gratified that it has become my duty on the present occasion, to commit to your care, as the representative of our common friend, Professor Dumont, the Wollaston Gold Medal, which has been awarded to him by the Council for his Memoir on the geological constitution of the province of Liége, published at Brussels in 1832.* The grounds

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justice would undoubtedly be rendered to the ideas of M. Poisson on the causes of the temperature increasing towards the inner portions of the crust of the globe; attention would be bestowed on the observations of Herschel, which lead to the supposition that our sun has not always acted with the same force; and above all, calculations would not be made of an unreasonable kind. If the proposition of internal incandescence were not so ardently demanded by the volcanists, it would be promptly confessed that it is not at all *demonstrated by the facts* cited in its favour; above all, it would be confessed, when use is made of thermometrical facts deduced from mines, pits, &c. for calculating the degree of heat of some internal part of our planet, that use is made also of a very gratuitous and rash supposition, viz. that the law of the increase of temperature deduced from the series of temperatures observed (a series truly ridiculous, when we reflect on the real dimensions of the globe), must be correct at every distance from the surface. Volcanos *prove* to us *directly*, that there are portions in the interior of the earth which are in a state of fusion; but these portions are comparatively extremely near the surface, and there are not sufficient reasons for believing that they are not isolated. Besides, the changes of level of *some parts* of the surface, *cause it to be presumed* that the temperature of the corresponding portions in the interior is variable, but that it increases as well as diminishes. As to the actual state of the central portion of our globe, it must be confessed, that we are in perfect ignorance in respect to it, and that, as much after being in possession of observations on the temperature of some points of the crust of our planet, as before that time."

* The title of this work is, "*Mémoire sur la Constitution Géologique de la Province de Liége, en reponse à la question suivant: 'Faire la description géologique de la Province de Liége; indiquer les espèces minérales et les fossiles accidentels que l'on y rencontre, avec l'indication des localités et la synonymie des noms sous lesquels les substances deja connues ont été décrites.' Qui a remporté le prix au concours de l'Académie de Bruxelles, en* 1830. *Par A. H. Dumont.*" 4to, pp. 374. With a geological map, a table of formations, and sections.—Edit.

of this our tardy recognition in 1840 of the merits of a work, published so long as eight years ago, are the same that, in 1830, prompted the judges appointed by the Academy of Brussels to select this memoir as most worthy of the prize, then proposed by that Academy, for the best geological description of the province which has formed the subject of M. Dumont's successful labours. In the work thus doubly crowned, the author has described the mineralogical and zoological characters of the rocks which occupy this district; he has also determined, in minute detail, the relative place in the order of succession, and the superficial extent of each subordinate division of the several formations; and has illustrated the same by an accurately coloured geological map, and by coloured sections, shewing the general disposal of the strata in their original order of superposition, and the extraordinary derangements and disturbances that have subsequently thrown them into a state of almost inextricable confusion. In the execution of this work, M. Dumont has evinced unusual powers of discriminating and accurate observation, combined with a high capacity of reducing the minutiæ of local details under the dominion of enlarged and masterly theoretical generalizations. Advancing, at the early age of twenty-one, to a task of gigantic labour, in a region where the unheard of disturbances, and almost incredible complexity of its component strata, had baffled the sagacity of the most experienced geologists, this extraordinary youth at once withdraws the veil of confusion which had hitherto disguised the stratigraphical arrangement of his native province; and, as it were, by an intuitive touch, reduced to order the entangled and almost incredible phenomena of dislocation, contortion, and inversion, which had perplexed his predecessors in the same field of observation. In addition to the scientific value of M. Dumont's exact and laborious researches, in illustrating a high and difficult problem in positive geology, his work assumes a place of great statistical and commercial importance, as describing the structure and contents of a rich and productive carboniferous district, containing eighty-three beds of valuable coal; and its practical utility has been fully shewn, by the fact of a second edition having been required to supply the demands of

the landed proprietors, and persons practically interested in the operations and products of the coal mines. The Geological Tribunal of Brussels, including the highly distinguished geologist Omalius d'Halloy, at once appreciated duly, and rewarded as they deserved, these brilliant discoveries; but the phenomena, represented on M. Dumont's map and sections, were so unusually complicated, and apparently impossible, that the geologists of England could not but forbear to admit their reality, until it was fully confirmed by our personal examination, with the aid of that new light which M. Dumont's discoveries had thrown upon them. The result of such inquiry has been a full corroboration of M. Dumont's representations; and at this late hour we come forward to render him the homage of our tardy but sincere acknowledgments, a duty too long delayed from the exercise of precaution in its administration, but for this very reason now become more urgent when the grounds for conscientiously discharging it have passed the ordeal of severe and critical investigation. It is for this great work, then, on the geological constitution of the province of Liége, such as in 1832 it issued from the hands of a young, and then unknown, individual, and apart from any more recent attempts to identify the Belgian formations with those of England, that our Society has awarded to M. Andrè Hubert Dumont their gold Wollaston Medal for the present year, in testimony of their admiration of the almost precocious talents then displayed by him, and of their sense of his worthiness to fill the distinguished scientific position to which he is now advanced, as professor of mineralogy and geology in the College of Liége."

On presenting the year's proceeds awarded to Mr Sowerby, Dr Buckland said:—

"It is with no small pleasure that I rise to perform the duty of placing into your hands the award that has been made to you, by the Council of this Society, of one year's interest of the Wollaston Fund, in order to facilitate the continuation of your researches in mineral conchology. The services are great which have been rendered to geology by the extremely useful and well-timed work on fossil shells, which was many years ago begun by your excellent father, and continued by him to

the end of his life, and has been since conducted by yourself, and the association of his name with that of Dr Wollaston recalls to my mind, as it must to the minds of most of my hearers, pleasing and grateful recollections of the benefits which, during their lives, they both conferred on this Society, and which their works will have extended to our contemporaries and successors in this department of scientific inquiry. It was your father's peculiar merit to be one of those accurate and enthusiastic observers of nature who have, in modern times, contributed so much to remove from science the rugged and austere aspect under which it used to be presented; and who, by facilitating to every one the means of advancing pleasantly in its pursuits, have, in an essential manner, promoted and given popularity to the sciences of botany and conchology. It is to mineral conchology, which he has so especially promoted, that we, who are occupied with the investigation of the structure of the earth, have in modern times been mainly indebted for evidences which have led to the establishment of many of the most important stratigraphical distributions that have arisen from discoveries as to the successive changes in animated nature, made known to us by the study of fossil shells. It was on this foundation that Cuvier and Brongniart established their important divisions of the marine and fresh-water strata of the tertiary formations, still more minutely distributed by Mr Lyell into the Eocene, and Pliocene, and Miocene series, according to their relative numbers of extinct and recent species of fossil shells. It was on a similar foundation that Smith rested his identification of the secondary formations of England by their fossil remains. It is on the same basis of conchological evidence that Mr Murchison has founded his fourfold subdivisions of the silurian portion of the transition-rocks; and it is chiefly to the illumination which this branch of palæontology has shed upon the changes that took place on the surface of the earth, whilst its strata were in the process of formation, that we owe the rapid advance in geological knowledge which has been made since the commencement of the present century. To this rapid progress, arising from the introduction of the evidences of mineral conchology, your publications, and those of your family, have largely contributed.

You have further co-operated materially in advancing our inquiries, by your personal assistance, at all times cheerfully and liberally rendered to all your fellow-labourers, in the same field of scientific research, who stood in need of your aid, for the elucidation of minute distinctions in the characters of fossil organic remains, which have at this time become so important an element in geology. The volumes of the Transactions of this Society, and other publications by many of its members, including myself, bear further testimony to the value of your labours in illustrating our works with drawings and engravings of fossil shells and plants, expressing their characters with a degree of accuracy and truth, which no pencil or burine but those of a scientific artist could possibly accomplish; and I am sure I give utterance to the feelings of many of our fellow-labourers, now around me, when I thus publicly acknowledge the services you have rendered, both to ourselves and to the science we cultivate; and testify our satisfaction at the present public recognition of your valuable services by the Geological Society of London."

Arrangement of Minerals, founded upon Physical and Chemical Characters.

CLASS I.

Characters of the Class.—Specific gravity less than 3.8. No bituminous smell. If solid is sapid.

Order I. Gas. Order II. Water. Order III. Acid. Order IV. Salt.

CLASS II.

Characters of the Class.—Specific gravity more than 1.8. Tasteless.

A.—Haloidal Minerals.

Tasteless compounds of Earths and Acids, and of Metals and Acids.

SUBCLASS I. HALLITE.—*Saline Minerals.*

Tastless compounds of Earths and Acids.

Order I. KUPHALLITE.—*Light Saline Minerals.*—Genera, Gypsum, &c.

Order I. BARALLITE.—*Heavy Saline Minerals.*—Genera, Heavy-Spar, &c.

SUBCLASS II. HALOCHALCITE.—*Saline Ores.*

Tasteless compounds of Metals and Acids.

Order I. BARALOCHALCITE.—*Heavy Saline Ores.*—Genera, Sparry Iron, &c.

Order II. KUPHALOCHALCITE.—*Light Saline Ores.*—Genera, Malachite, &c.

Order III. MICALOCHALCITE.—*Micaceous Saline Ores.*—Genera, Uran-Mica, &c.

Order IV. KERALOCHALCITE.—*Corneous Saline Ores.*—Genera, Horn-Ore, &c.

B.—EARTHY MINERALS.

Minerals in most cases composed of one Earth or more, frequently coloured with metallic oxides, especially those of iron.

Order I. MICA. Order II. GRAPHITE.* Order III. STEATITE. Order IV. SPAR. Order V. GEM.

C.—METALLIFEROUS MINERALS.

Minerals in which Metals, generally the chief constituents, are in the native state, or combined with Oxygen, Sulphur, or Arsenic.

Order I. ORE or OXIDE. Order II. METAL or NATIVE METAL. Order III. PYRITES. Order IV. GLANCE. Order V. BLENDE. Order VI. THIOLITE.

CLASS III.

Characters of the Class.—Specific gravity less than 1.8. If liquid the smell is bituminous. If solid, is tasteless.

Order I. RESIN. Order II. COAL.

* The Order Graphite is placed provisionally among the Earthy Minerals.

Notice respecting the Fall of an Aerolite, near Juvinas, in the Department of the Ardêche, in France, witnessed on the 15th June 1821. By Professor FORBES. Communicated by the Author.

Last summer, I spent several days in the remote little town of Montpezat, in the Department of the Ardêche—formerly the Haut Vivarais—examining the interesting extinct volcanoes in the neighbourhood. I accidentally learned that I was within a few miles of the spot where a meteoric stone had been *seen to fall* not many years ago, and resolved to devote a day to endeavour to obtain an account of the phenomenon from eye-witnesses, and to procure a specimen of the mass.

The place mentioned to me as that where the stone had fallen, was the hamlet of Libounet or Libounez, between St Pierre Colombier, in the valley of Burzet, and the village of Juvinas, the capital of the commune. After a walk of five or six miles, through a country of exquisite beauty, I arrived at Libounez, situated among trees, on the northern slope, but very near the bottom of a steep valley, of no great extent, which communicates with that of Burzet. Every body in the neighbourhood speaks of the fall of the meteor as an event of yesterday, and one never to be forgotten. The field where it fell was immediately shewn to me, a small enclosure just below the village. I inquired for the actual spectators of the fall. Dolmaas, who had been mentioned to me, was dead; but with some difficulty I found two brothers, named Serre, who were working with some others at their potatoes when the stone fell amongst them. One of these men gave in his Patois a most animated account of the scene, and their terror. With the aid of an interpreter, I extracted the following particulars, agreed upon by those present whom I questioned.

The stone fell in June 1821—sky clear and wind north—at $4\frac{1}{2}$ P.M. A long roll was heard, then an explosion like a cannon, five minutes before the stone fell. It touched the ground within a few feet of them, which it perforated to a depth of seven palms ($5\frac{1}{4}$ feet), *in a vertical direction*. It

burnt the ground to a cindery state. No lightning accompanied the noise or the fall. The men were frightened, but not stunned. The noise was heard to a great distance; a man present said he had heard it at Argentiere (ten miles in a straight line). The people thought it was the devil had fallen, and did not venture to dig up the stone for seven days, when it was sprinkled with holy water by the priest. The hole was exactly the size of the stone. There was no scattering of earth. The direction of the stone's flight was from the north-west: (this seems at variance with the assertion, that it penetrated the ground vertically). The stone had wedged itself between two others, and could not be removed without breaking it. It weighed 220 pounds, as I learned from the man who weighed it. It was sold for six francs!

I then went to Juvinas to question the Maire as to the *procès verbal* stated to have been drawn up on the occasion. I was a little amused at the circumstances and habitation of that dignitary, being an ordinary peasant, living in a wretched hovel. Where the *Archives* could be was a puzzle; and, on inquiry, it appeared that the said *procès verbal*, not having been kept in duplicate, had been sent to the Prefecture at Privas.

Being determined to gain every possible information, I subsequently went on purpose to Privas, the *Chef-Lieu* of the department, and through the civility of M. Berlié, géométre-en-chef du Cadastre, I obtained a sight of, and was permitted to copy, this quaint and amusing document, which has, it appears, been already published in one of the French journals.

It appears from other accounts, that the meteor was seen by many persons at Viviers (twenty-five miles from Juvinas) descending apparently on the Coirons (N.W.), with the appearance of a brilliant star. After disappearing, it left a smoky trace.

Another eye-witness, M. Jules de Malbos, at Bariat, eight leagues from Juvinas, heard two detonations; and happening to be looking up, watching his men stripping mulberry trees, he saw a luminous meteor *fall perpendicularly*. It left a ribband of smoke, which remained tranquilly for more than ten minutes (Ann. de Chimie et de Physique, tom. xvii.).

Vauquelin gives the following analysis of 5 grammes :—

Silica,	2.00
Alumina,	0.67
Iron and Manganese,	1.35
Lime and Magnesia,	0.40
Sulphur, Chrome, Copper, Potassa.	0.58
	5.00

It differs from other aerolites chiefly in containing no nickel, but traces of copper and potassa (Ann. de Chimie, xviii. 421). It is not magnetic.

I find its specific gravity to be 3.122 at 52°.

Observations on the Geology of Scandinavia and Spitzbergen. By M. Eugene Robert. Communicated to the Imperial Academy of Sciences of St Petersburgh.*

M. de Struve communicated, on the 11th January, the following abridged view of the geological observations made by M. Eugène Robert in Scandinavia and Spitzbergen, during the expedition of the French corvette *la Recherche* in 1838.

This geologist first directed his attention to the constitution of the islands of Denmark and Holstein. He ascribes their formation to marine currents, a cause still in operation, and which may ultimately wholly obliterate the Sound and the Belts, and by so doing, convert the Baltic into a Caspian Sea. He adduces in support of his theory, 1*st*, The point of Skagens Udde in Jutland, which perfectly represents, in the Gulf of Christiania, one of those salient angles formed by the land gaining upon the water in proportion as the latter recedes. 2*d*, The Island of Jomfruland on the east side of Norway, which is gradually continuing to extend itself, and represents very correctly, by its lengthened form and composition, the Aasar of Sweden, which M. Robert ascribes to the same phenomenon. With regard to the great blocks contained in these alluvial deposits, he is of opinion, that they have been

* "L'Institut," 16th January 1840.

transported by ice to the plains they now occupy. According to M. Robert the greyish sandy argillaceous soil of Hamburgh, Zealand, the rich valleys of Norway, particularly the basins of Christiania and Drontheim, are assignable to the same cause, namely, the disintegration of the clay-slate rocks, so abundant in this country. The bottom of the sea, where it washes these coasts, and even at a distance from them, is likewise composed of the same elements. M. Robert then cites a great number of facts which tend to shew that all the coasts of Norway and Sweden exhibit marks of the sea to the height of 600 or 700 feet, as proved by the existence of polished and rubbed blocks of a rounded form, or by the deposition on their surface of fossil shells, the greater part of which still live in the sea. He particularly mentions, in support of his opinion, a secondary limestone near Christiania, pierced by the *Saxicava rugosa*, at the height of 400 feet above the present level of the sea, and beds of shells at Krageroe (east side of Norway), at the North Cape in many places, and at Soederhamn (on the east side of Sweden). He observed likewise, near the North Cape, numerous terraces or old banks, which seem to indicate that the phenomenon of the elevation of Scandinavia, or the sinking of the waters of the sea, was occasionally subject to intermissions. Having observed, moreover, indications of the presence of the sea in the centre of the tract which separates the waters of the Icy Sea from the Baltic, M. Robert has also come to the opinion, that, formerly, Norway and Sweden formed only an island or a great archipelago. M. Robert is much more inclined to favour the opinion, that the waters have receded in the northern parts of the globe, than that there has been an elevation of the soil, seeing, that of all the countries with primitive mountains, Scandinavia, with an extent of 15° of latitude, is perhaps the only one which has never been shaken by an earthquake, and which presents neither volcanic deposits, properly so called, nor the least appearance of a thermal spring.

According to the same geologist, the gigantic wall which forms the North Cape, is composed of a diallagitic syenite, or of the *norite* of Esmark: but he mentions particularly a small

nameless islet placed more to the north, which being composed of gneiss, will leave to that rock the glory of forming the extreme northern boundary of Europe.

At Hammerfest, M. Robert found an extensive deposit of volcanic scoriæ coming, no doubt, from Iceland, and, among the drift wood from America and Russia, the fruit of *Mimosa scandens*, an occurrence only of interest by illustrating the history of currents.

From Hammerfest, the French expedition repaired to Spitzbergen, which M. Robert found to belong almost entirely to the secondary and transition periods, notwithstanding the pointed form of its mountains, which caused the Germans, as is well known, to name the country Spitzbergen. This country must have undergone some great commotion, judging from the upraising of the strata of the coal-formation; and it presents, as well as Norway, evident marks of having been covered by the sea. M. Robert thinks, that the appearance of this island is contemporaneous with that of the mountains of Scandinavia, that, in a word, they are sisters. Here, in particular, he noticed high mountains of calcareous breccia (*Nagelflue*), and others composed of a great number of strata containing black bituminous coal, and impressions which appear to have belonged exclusively to large monocotyledonous and herbaceous or annual plants, but there were no impressions of ligneous plants, such as tree-ferns. On this subject M. Robert remarks, that this fact, if fully established, goes to support the theory of the primitive heat of the globe, for while the present temperate regions were capable, under this influence, of causing the development of large palms, tree-ferns, &c., as is proved by the fossils, nothing could grow, at the same period, in Spitzbergen, but herbaceous plants, the six months of night inevitably producing putrefaction, and, consequently, the absence of ligneous vegetables. M. Robert has likewise observed in Spitzbergen, a limestone with spirifers, &c., and has seen a hypersthenic syenite in numerous places which is covered by a steatitic slate without any kind of fossil. M. Robert has paid particular attention to the glaciers of this island, which are as numerous as the valleys, and run directly

to the sea. He is of opinion, that, the greater part of the floating ice of the northern sea comes from these glaciers; he even thinks that there are no fields of ice in the ocean but when there is land in the neighbourhood, and that it may easily happen, for this reason, that there should be none at the pole itself. On returning from Lapland, M. Robert visited the copper mines of Kaafiord, situated in the Altenfiord, and found them very rich in produce, and also in minerals. One of them is in the norite of Esmarck, and the other in a calcareous breccia. The great plateau of Lapland was found to be entirely composed of gneiss, and covered with a deposit of magnetic sand, which he ascribes to its having been the site of a large lake. He has likewise ascertained that the sand of the great rivers of this country, such as the Alten and the Muonio, contain a great deal of oxidulated magnetic iron. He found this metal also in a vegetable earth, and thinks that, for this reason, the most scrupulous attention is demanded in making observations with the magnetic needle. Finally, M. Robert has directed his attention to the transported matters of the soil of Scandinavia, or, in other words, the erratic boulders; instead of considering them as such, or referring them to any violent agency, he is of opinion that, in most cases, they are not remote from their original site, and that their rounded form, and occurrence in collected masses, are owing to causes still operating on the globe, such as the continued washing of the sea, torrents, &c. Neither does he believe that the worn appearances *(rognures)*, sometimes noticed in the Scandinavian rocks, are owing to the alleged phenomenon, very gratuitously dignified, according to him, with the title of diluvium.

Notice of Greenockite, a new Mineral Species of the Order Blende.

Description.—Form, prismatic? Colour, between honey and orange yellow, rarely inclining to brown. Colour of the streak, reddish orange. Lustre, shining, resinous, somewhat

inclining to adamantine. Semi-transparent to semi-translucent. Hardness, about 3.5. Specific gravity, 4.8.

Chemical composition.—It is a proto-sulphuret of cadmium, and itsformula is Cd S.

Geographical and Geognostical Situations.—It occurs near Bishopton in Renfrewshire, in a porphyritic and amygdaloidal trap rock, containing crystals of felspar and amygdaloidal portions of calcareous spar, green earth, &c. Cavities lined with prehnite are often met with; and the Greenockite is found either superimposed on the botryoidal surface of that mineral, or disseminated through its fibrous mass.

History.—This mineral was first noticed as an undescribed substance by Lord Greenock, in compliment to whom we have named it Greenockite. We are informed by Mr Brown of Lanfyne, that he has had specimens of it in his possession for many years, and that in his collection it was arranged among the zinc blendes. The Greenockite is a species of a new genus (the 6th in number) of the 5th order of the 2d class of the systematic arrangement, which we intend soon to lay before our readers, and of which a tabular view appears in our present number. It is to be regretted that the limited number, and the small dimensions, of the specimens hitherto obtained, do not admit of the characters of this beautiful and interesting substance being very fully determined. The crystals are short 6-sided prisms, with truncated 6-sided pyramidal terminations; and two pyramids of different degrees of acuteness have been noticed. These prisms have, at first sight, much the appearance of regular 6-sided prisms, but are probably compound forms. The faces of the more acute pyramid are transversely streaked.

Mr Brooke of London, to whom we sent some of the crystals for examination, has kindly sent us the following sketches of two of the crystals, with the accompanying measurements and remarks. He says, "the form appears to be a regular 6-sided prism, but I suspect that it is derived, like sulphate of potash, from the intersection of three rhombic prisms. I have measured the broken crystal, and find the angles to be very nearly

"*a b* 136° 40′; *a c* 118°; *a d* 90°; *b e* about 176°, a salient angle resulting, I conclude, from the composition of two crystals.

No. 1.

No. 2.

"No. 2 is evidently a compound crystal, but I must have better means than the single crystal has afforded, to be enabled to discover the primary form. I do not observe any rings in looking perpendicularly through *a* and the parallel plane, which, although the crystal is only translucent, I think I should do if the primary form were a rhomboid, or rhombohedron as now called. The lines represented on the faces of No. 2. present bold salient and re-entering angles. The only appearance of cleavage, I have found in breaking the small crystal, is parallel to the lateral planes *d* of the hexagonal prism, but I am not certain that these are more than planes of composition of intersecting crystals."

Chemical Examination of Greenockite, or Sulphuret of Cadmium. By Arthur Connell, Esq., F.R.S.E. (Communicated by the Author).

A fragment of Greenockite, heated in a glass tube, decrepitated and acquired a fine carmine red colour, and, on cooling, recovered its yellow tint. At a red heat it did not fuse or volatilize. It gave off no moisture.

In powder, it was readily soluble in heated muriatic acid, with strong odour of sulphuretted hydrogen. Carbonate of soda caused a white precipitate soluble in ammonia. The muriatic solution, by evaporation, afforded a white prismatic crystallization, not deliquescing in an ordinary atmosphere. This character distinguished the mineral from zinc-blende, which it resembles, and suggested the idea that it might be sulphu-

ret of cadmium, as the change of colour, by heat, seemed also to indicate. This supposition was confirmed on finding that the precipitates by potash, and by carbonate of ammonia, were not dissolved by excess of the precipitants, and that sulphuretted hydrogen threw down, from a muriatic solution, with excess of acid, a fine yellow precipitate. After the current of sulphuretted hydrogen no longer caused a precipitate, the solution was neutralized by ammonia, when a few dark flocks of sulphuret of iron fell. The current of sulphuretted hydrogen was then removed, but no farther precipitate was occasioned; shewing the absence of zinc, a conclusion farther confirmed by finding that the excess of potash, and of carbonate of ammonia, used as precipitants, took up nothing. The muriatic solution of the mineral farther gave a yellow precipitate with hydrosulphuret of ammonia, and white precipitates with prussiate of potash, oxalate of ammonia, and phosphate of soda, and no precipitate with sulphuric acid. A piece of zinc threw down reduced metal as a grey ramification.

These various reactions left no doubt that the mineral under examination was sulphuret of cadmium, that it contained no zinc, and that the only impurity which could be detected was a slight trace of iron. The observations were farther confirmed by comparative trials made on a solution of metallic cadmium.

I was indebted to Lord Greenock for a crystal of the mineral sufficiently large to enable me to take its specific gravity, and also determine the proportions of the constituents. The specific gravity of this crystal was found to be 4.842 at 60° F Previous to analysis, the dark and opake portions were separated.

3.71 grains of the transparent portion of the mineral were reduced to somewhat coarse powder, and fuming nitric acid was poured on them drop by drop in a deep flask, and the whole digested with excess of acid, till all the sulphur which had separated was dissolved. Water was then added, and the sulphuric acid thrown down by muriate of barytes. The sulphate of barytes, after being well washed with hot water, was dried and ignited; and weighed 6.07 grains, equivalent to .837 grains of sulphur.

The excess of barytes was then removed from the liquid, after concentration by heat, by sulphuric acid. After again concentrating, carbonate of ammonia was added in excess. The carbonate of cadmium was separated by filtration, and well washed, dried, and ignited. The oxide of cadmium thus obtained had an ochre-yellow colour, and weighed 3.28 grains, equivalent to 2.868 of cadmium. A little of it dissolved in muriatic acid was entirely taken up by excess of ammonia.

The filtered liquid was then evaporated by heat, but no precipitation had taken place when all smell of carbonate of ammonia had disappeared; thereby confirming the previous observations as to the absence of zinc. The evaporation was carried to dryness, and the ammoniacal salt driven off by heat. A residue of .04 remained of a reddish-white colour, which, in so far as its small quantity permitted examination, was found to be, in part at least, a subsulphate of iron, insoluble in water, and scarce soluble even in acids, till previously boiled with potash; but, as the proportion of its constituents could not be determined on so little material, the iron could not be computed in any other way than by stating it as a trace in the mineral, its amount, on any view, being very small; and if more than such, this was not the stage of the analysis in which it should have been obtained.

We have thus, in the 3.71 grains of the mineral under analysis,

Sulphur, . . .	.837	22.56
Cadmium, . . .	2.868	77.30
Iron, traces.		
	3.705	99.86

which agrees completely with the theoretical composition of

1 atom Sulphur, .	201.16	22.40
1 atom Cadmium, .	696.76	77.59
	897.92	99.99

The mineral is thus a protosulphuret of cadmium, and its formula C*d*S. It is, so far as I know, the only *separate* ore of cadmium hitherto discovered.

It is readily distinguished by its blowpipe reaction from zinc-blende, the mineral which ranks nearest it in the system. From its decrepitating property, it is difficult to act on it *per*

se on charcoal; but when this can be accomplished, the usual yellowish-red ring, arising from the oxidation of sublimated cadmium, is formed around the fragment. When mixed with soda and acted on, on charcoal, this ring continues to be formed to the last, without any of the white sublimate which zinc-blende affords. With borax it yields a transparent yellow glass.

New Observations on the Effects of Veins of Quartz. By M. Rozet. (Communicated to the Academy of Sciences.)

In a memoir on the mass of mountains which separates the Loire from the Rhone and the Saône, previously presented to the Academy, I shewed that quartz forms conical eminences at a considerable number of points between the base of Mont-Pilat, in the *Departement de la Loire*, and near Avallon, in the *Departement de l'Yonne*. These cones have pierced the porphyry, granite, gneiss, slate, and carboniferous formations. They are every where accompanied by numerous veins of quartz, of the same nature as that of the cones, and which traverse the various rocks in all directions, sometimes even penetrating to lower parts of the lias. These veins, while flowing out at the surface of the granite, of which they have agglutinated the debris, have formed those remarkable rocks the *arkoses*, which M. de Bonnard was the first to notice. During that operation they have produced a number of curious effects, which I have described in my memoir; but the environs of Semur and Brionnais (*Saône-et-Loire*) having this year presented to my observation phenomena of another description, and of a certain degree of importance, I request the permission of the Academy to communicate a succinct account of them.

Around the village of Saint-Christophe in Brionnais, we see making its appearance a granite containing large rose-red crystals, which is decomposed with facility, is quarried at several points for sand, and is traversed by veins of hyaline quartz, which passes into calcedony. This granite is covered by a thick bed of reddish marls and arkoses, which supports the largely-developed lias of that part of the country. At 500

yards to the north of the village, on the new road to Charolles, we observe the limestone, containing Gryphea arcuata, directly covering the granite, and so connected with it, that there results an insensible, but mechanical passage, from the one to the other. The granite, in its upper portions, being impregnated with limestone, effervesces with acids; and the limestone, in its inferior portions, contains so large a quantity of crystals of felspar, that there results a very singular calciphyre. Near the bottom, the felspar is much more abundant than the limestone, but it diminishes as we proceed upwards, and, at a yard and a-half above the granite, the crystals of felspar have disappeared. In all this portion of the lias-formation there is not a trace visible of organic remains. But in the beds immediately above, and which are intimately connected with the calciphyres, we find abundance of the Gryphea arcuata and other characteristic fossils. In the portion containing the crystals of felspar, the stratification of the limestone is very confused, and, indeed, frequently has entirely disappeared; the limestone itself is much altered; its density is sensibly augmented; it has assumed a yellowish colour and a crystalline aspect; at a greater distance it has become brown, with yellow spots; this last variety contains few crystals of felspar; in the two modified varieties we remark a great number of quartz veins, which lose themselves insensibly, and never rise to the shelly beds; below, the veins become connected with large trunks which traverse the inferior granite, and are true veins, identical with those which we notice in the whole granitic mass. It is the eruption of the quartz, evidently contemporaneous with the first period of the lias deposit, which has thrown the crystals of felspar into the limestone, and amalgamated with it the granite which had been cooled for a long period previously. In all this portion the limestone has become magnesian, and especially in the neighbourhood of the veins of quartz. The brown variety, which is the most altered, and in which the veins of quartz are the most numerous, is also the most magnesian. I found in it nearly a third of magnesia, of iron, and of silica, a fact which has been confirmed at the School of Mines by M. Elie de Beaumont.

Here, then, we have an example of limestones rendered

magnesian by the influence of veins of quartz, which presents some analogy to the mode of formation of dolomites by the influence of black porphyries, a phenomenon pointed out a long time ago by Von Buch in his beautiful memoirs on the subject. At St Christophe we have another fact, viz. the intimate union, without any intermediate rock, of granite, the most ancient Plutonic rock, with the lias, a Neptunian rock, filled with marine shells. Facts of this kind, whose cause was unknown, have often been cited, to prove that the granite was of the same epoch as the jurassic formation. What I have now stated, proves that observers may be deceived in this respect. (*Comptes Rendus de l'Academie des Sciences*).

Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, and chiefly in the Royal Botanic Garden. By Dr GRAHAM, Professor of Botany.

10*th March* 1840.

Bouvardia splendens.

B. *splendens*; foliis ternis, raro oppositis, lanceolatis, acuminatis, utrinque scabris; stipulis lacineato-subulatis; ramis trigonis subglabris; stylo exserto; corollæ tubo intus barbato.

Bouvardia splendens, *Bot. Mag.* 3781.

DESCRIPTION.—*Shrub* with long, slender, diffused branches, grey and cracked when matured, when young trigonous, subglabrous, coloured on their upper side, green on the lower. *Stipules* subulate, occasionally cut, elongated, adpressed. *Leaves* ternate, rarely opposite, lanceolate, acuminate, strongly veined, scabrous on both sides, bright green above, pale below. *Corymbs* terminal, with three lateral branches and a leading stalk, which is once or oftener divided in a similar way. *Calyx* green, slightly scabrous: tube adherent; limb 4-parted, segments subulate, diverging, and having minute intervening teeth. *Corolla* of uniform very bright vermilion colour, acquiring a faint lake tint, slightly scabrous; tube of equal diameter above and below, bluntly 4-angled, nearly five times longer than the calyx, bearded near its base within; limb 4-parted, segments ovate, spreading, subacute. *Stamens* four, included; anthers sessile, attached by their middle to the corolla at about three-fourths of its height; pollen yellow. *Stigma* bifid, fleshy, glandular, segments oblong, slightly divaricated at the apex. *Style* central, filiform, glabrous, exserted at the apex. *Germen* inferior, bilocular, shortly elliptical or obovate, compressed; placentæ large, central; ovules numerous, imbricated, winged all round, resembling concave scales.

There is no doubt great similarity between this and the *Bouvardia triphylla* of Botanical Magazine, but its freer growth, the much greater splendour of its vermilion colour, the narrower, more lanceolate, more acuminate,

more scabrous leaves, the greater length of its stipules, and its exserted style, seem to justify the separation of it from any variety of that species. It was obtained from the Chiswick Garden by Mr James Macnab in September 1838, and flowered very freely in the greenhouse of the garden of the Caledonian Horticultural Society in July and August. Mr J. Macnab does not know whence it was introduced; he has made many attempts, always unsuccessful, to propagate it by cuttings from the branches, but has found it very easily propagated by slips from the roots, not half an inch long, covered so as to leave the upper extremities only exposed, and level with the surface. He has distributed many plants obtained in this way under the name I have adopted. There is scarcely any thing more brilliant in cultivation than a large specimen covered with clusters of its most splendid flowers. The seeds formed, but did not ripen.

Erysimum Perofskianum.

E. *Perofskianum*; annuum vel bienne, setis bipartitis scabrum; caule simplice, foliis lanceolatis, remote denticulatis; pedicellis calyce duplo brevioribus; petalorum ungue calycem vix superante; siliquis setis bipartitis scabris, stylo teretiusculo pedicelloque quintuplo longioribus.

Erysimum Perofskianum, *Fisch. & Meyer.* Index quartus Sem. in Hort. Bot. Imper. Petropolit. ann. 1837.—Bot. Mag. 3757.

Description.—*Stem* ascending, weak, angled, simple. *Leaves* of nearly uniform green on both sides, scattered, lanceolate, remotely denticulate, attenuated at the base, recurved at the apex, with bipartite adpressed hairs; middle rib prominent behind, 2–4 lateral obscure nerves. *Spike* racemose, terminal, many-flowered, pedicels spreading. *Calyx* pale, erect, deciduous; sepals unequally bulging, and distant at the base, keeled at the apex, especially the two which are longer and narrower. *Corolla* of uniform orange colour; laminæ of the petals suborbicular, spreading at right angles, at first slightly concave, afterwards reflected, shorter than the slender wedge-shaped claws, which are scarcely longer than the sepals. *Stamens* distinctly tetradynamous, the shorter equal to the claws of the petals. *Hypogynous glands* dark green, indented by the claws of the petals, secreting much honey. *Pistil* longer than the stamens; germen spreading, 4-sided, slightly pubescent, having many ovules in one row; style less than half the length of the germen; stigma capitate, cleft across the vertex. *Siliqua* compresso-four-sided, rough with bipartite adpressed hairs, crowned with the persisting style, which is nearly as long as the pedicel, and 4–6 times shorter than the siliqua.

Seeds of this very pretty plant (which belongs to De Candolle's 2d section of the species, Cuspidaria) were received at the Royal Botanic Garden, Edinburgh, in May 1838 from Dr Fischer, and the plants raised that season flowered in a cold frame in May 1839. They ripened seed. The species is a native of Cabul.

Gardoquia multiflora.

G. *multiflora*; fruticosa, subglabra; foliis petiolatis, ovatis, obtusiusculis, subcrenatis, basi rotundatis, viridibus, subtus pallidis, floralibus conformibus; verticillastris laxis, subsecundis; cymis pedunculatis, vix dichotomis; calycis glabriusculi dentibus acutis, fauce intus nuda; corollis calyce triplo longioribus.—*Benth.*

Gardoquia multiflora, *Ruiz et Pavon*, Fl. Peruv. et Chile ined. 4. t. 495, f. *a.*—*Benth.* Labiat. 398.—Bot. Mag. 3772.

Rizoa ovatifolia, *Cav.* Ic. Rar. 6. 56, t. 578.

Description.—*Shrub* erect, much branched, as well as the stem; branches spreading, obscurely 4-sided, bark cracked. *Leaves* petiolate, ovate, re-

flected in the edges, glabrous on both sides, of lively green above, dotted and rather paler below, where also the middle rib and oblique veins are prominent. *Peduncles* (3 lines long) axillary, solitary, collected towards the extremities of the branches, 3-flowered, glabrous; pedicels rather longer than the peduncle, the lateral ones having a small adpressed bractea on their outer side at the base, and two opposite ones below the middle, the central one being without any bracteæ. *Flowers* handsome, pendulous. *Calyx* (¼th of an inch long) slightly curved, green, sprinkled with glands, and very obscure pubescence, teeth erect, nearly equal, the uppermost rather the smallest. *Corolla* five times as long as the calyx (at least in cultivated specimens) red-purple; tube clavato-funnel-shaped, slightly compressed laterally, slightly pubescent on the outside, hairy within, especially on the lower side and towards the limb; limb 2-lipped; upper lip slightly reflected, of two rounded teeth, lower lip of three, more recurved, linear segments. *Stamens* exserted, straight, distant, the two longer projecting forwards from the centre of the flower, the two shorter parallel to and above them; filaments colourless, adherent to about the middle of the tube, above which they are free, and at the point of separation the corolla projects in little external corresponding eminences; anthers dark lilac, of two much divaricated lobes, opening along the vertex; pollen white. *Pistil* equal to the longest stamens, pale lilac towards the top; stigma bifid, segments nearly equal, subulate, curved outwards; germen-lobes erect, placed upon a fleshy disk, which is longer than the lobes.

This very ornamental plant was received at the Garden of the Caledonian Horticultural Society from Mr Atkin, nurseryman, Northampton, in June 1838, and produced in the greenhouse a succession of flowers during nearly the whole summer and autumn of 1839. It very much resembles *Gardoquia grandiflora*, but may be distinguished from this especially by its foliage being perfectly glabrous even when young, by its patent, not erect branches, and by its shorter style.

Osbeckia canescens.

O. *canescens*; caule fruticoso, erecto; foliis cordato-ovatis, obtusis, utrinque tuberculatis, subtus incanis; paniculis terminalibus axillaribusque; bracteis ovatis, caducis; floribus erectis; calycibus ovatis, 5-fidis, tuberculato-incanis; petalis obovatis, glabris; staminibus 10 fertilibus, connectivis inæqualibus.

DESCRIPTION.—*Stem* (7 feet high) weak, requiring support, 4-sided, rough, hoary: branches erect. *Leaves* (3 inches long, 1¾ broad) shortly petiolate, opposite, decussating, subcordato-ovate, blunt, 5–7-nerved, spreading, deflected when old, tubercular on both sides, bright green above, hoary below. *Peduncles* axillary and terminal, often divided, many-flowered, in panicles at the extremity of the branches. *Bracteæ* solitary at the origin of the pedicels, concave, ovate, caducous, articulated at the base. *Calyx* on the outside, like the pedicels, and the greater part of the plant, hoary, within glabrous; tube ovate; limb 5-partite, segments ovato-deltoid, spreading, with alternate external filamentous erect appendages. *Flowers* (1½ inch across) very numerous, very handsome; petals obovate, scarcely emarginate, cuneate towards the base, glabrous, reddish-lilac above, paler below, inserted into the throat of the calyx. *Stamens* 10, all fertile, declined; filaments glabrous, about half as long as the petals, inserted into the throat of the calyx; anthers purple, compressed laterally, undulate along the face, nearly straight, opening by a pore at the apex; connective bright red, in five, in which the anthers are erect, having a sessile emarginate yellow auricle at the base, in the other five twice as long as the anthers, and ascending, the auricle being elongated submarginate and yellow. *Pistil* about as long as the longest stamens; stigma blunt, small, greenish; style glabrous, declined in the opposite direction from the stamens, very slightly ta-

pering upwards, articulated at the base; germen hoary, naked and blunt at the apex, included within the calyx-tube, of five blunt lobes, adhering to the calyx at the angles only, 5-locular, dissepiments alternating with the lobes; placentæ central; ovules numerous.

This is an extremely handsome plant, and flowers very freely in moderate heat in July and August. We received it at the Botanic Garden, Edinburgh, from Berlin, under the name adopted, but I have not seen any account of it published, nor was its native country communicated to me.

Proceedings of the Royal Society of Edinburgh.

1889, *December* 2.—Sir T. M. Brisbane, Bart., President, in the Chair. The following Communications were read:—

1. Solution of a Functional Equation, with its application to the Parallelogram of Forces and to Curves of Equilibration. By Dr Wallace, Emeritus Professor of Mathematics.
2. On General Differentiation. Part I. By Professor Kelland.

December 16.—Sir T. M. Brisbane, Bart. President, in the Chair. The following Communications were read:—

1. Experiments on the Development and Growth of the Salmon, from the exclusion of the ovum to the age of two years. By Mr Shaw, Drumlanrig. Communicated by James Wilson, Esq.

The author, in this paper, gives an account of the continuance and confirmation of his experimental observations on the growth of salmon fry, as formerly communicated to the Society. He considers the objections made to his artificial ponds, in so far as they were supposed to afford an insufficient supply of food, to be without reasonable foundation, as these ponds actually abound with the ordinary insect food of fishes, and the young broods themselves correspond in every way to those of the same age in the natural streams and pools of the river. He had previously ascertained and stated, that young salmon remain in their native river for two years (under the names of parrs, pinks, fingerlings, &c.), and during that period, or till near its close, he applies to them the name of *parr*, as that by which they are usually designated in Scotland.

The hatching of the ova is regulated in a great measure by the temperature of the season. In severe winters, the principle of life is slowly developed. He found, by experiment, that spawn placed

in a stream of spring water of the average temperature of 40°, exhibited the embryo fish (visible to the naked eye) by the end of the 60th day, and was hatched on the 108th day after impregnation. Spawn deposited by the same parent on the same day *in the river* (the average temperature of which, for eight weeks, did not exceed 33°), did not exhibit visible young until the 90th day, and 131 days elapsed before their final hatching. Yet the departure of the more advanced fry *sea-wards* does not seem to depend on the variable time of hatching,—the first week of May, of the second ensuing season, being a very regular period for the main body, and on the occasion in question, large shoals of salmon fry were descending the river on the very day (10th May) on which the young of that year were only emerging from the ova. Yet the usual belief is, that they migrate in the course of the same spring in which they are hatched.

Mr Shaw's paper is accompanied by an extensive and varied collection of specimens, which exhibit the development of the fry from the period of hatching until the termination of the second year. A parr or young salmon twelve months old, measures about three and three-fourth inches in length, and exhibits what may be called the ordinary summer aspect. It corresponds in age and size with those which, in the natural bed of the river, are denominated "May parr," and it is these latter alone (with such as have been recently hatched) that are found in the river after the beginning of May, as about that period the two-year-olds all migrate to the sea. May parr are to be regarded as identical with the "Pinks of the river Hodder," allnded to by Mr Yarrell, but they are the produce of the preceding spring, instead of being, as usually supposed, only a few weeks old. They remain all summer, and throughout the ensuing winter, in the river, and, by an anomalous, or at least unusual, economy of nature, the males in the course of this early period become fit for generative purposes, and are seen to associate with the female adult salmon. A characteristic example of this stage was taken from the pond on the 14th November 1838, being then eighteen months old, and at that age all the males of the different broods exhibited a matured sexual character in relation to the *milt*, but none of the females of the same age shewed any signs of *roe*. Those in the bed of the river manifested the corresponding character of each sex,—that is, of maturity in the male, of immaturity in the female.

The great constitutional change which converts an old parr into a young salmon, usually takes place in the month of April of the second ensuing year. Specimens taken from the pond on the 5th of January 1839 measured six inches in length, and were twenty months old. They exhibited all the ordinary characters of the parr, commonly so called. But such as were allowed to survive till the 24th May, although they gained only half an inch in length, *cast off the livery of the parr*, and assumed that of the salmon,—this change consisting chiefly in the following particulars: the black opercular spots disappeared, the almost colourless pectoral fins

became suffused with an inky hue at their extremities, the broad perpendicular bars or blotches on the sides were effaced, and the prevailing hues of dusky brown and yellowish-white were converted into deep bluish-black above, and into silvery white below. Various other specimens exhibited by Mr Shaw exemplify a similar change, and some of these distinctly shew, as it were, the intermediate or transitionary state between the parr and salmon. The whole, however, belonged to broods which, as formerly explained by the author, were the original produce of adult salmon.

The observations hitherto reported were made in confirmation and continuance of those formerly laid before the Society by the same author. But the circumstance of the male parrs in a state of sexual maturity being found to accompany the female adult salmon while she deposits her ova in the river, suggested the idea of the following singular and successful experiment. In the month of January 1837, Mr Shaw took a female salmon weighing 14 lb. from the natural spawning bed, from whence he also took a male parr weighing one ounce and a half. With the milt of the latter he impregnated the ova of the former, and placing the spawn in the stream of his pond he watched its development, as he had that of the salmon spawn fecundated in the ordinary way, and found the hatching and subsequent growth in all respects to correspond. These experiments were repeated with the same results in the winter of 1838, and the parrs (taken from the river) which had been used as males, were kept alive till spring, when they assumed the migratory dress of young salmon. He also tried a corresponding experiment, by impregnating the ova of three adult female salmon taken from the river, with the milt of three parr bred in the confinement of the ponds, and the result was the same as to successful hatching and final growth,—this fact, moreover, shewing the constitutional strength of the pond-bred breed, and that they had not deteriorated or been altered in their natural character and attributes by confinement, as had been supposed. The specimens used in these experiments are exhibited to the Society, that all interested may satisfy themselves by ocular inspection.

One of these specimens is itself the produce of a male parr and female adult salmon; in other words, it is what naturalists (supposing the parr and salmon not to be identical) would call a hybrid or mule. Now, it is admitted by physiologists that the general rule in regard to these mixed productions, from kinds not specifically the same, is, that they do not breed; yet this male parr, originally produced from a parr and salmon, has itself become the parent of a numerous and healthy progeny of young salmon; and Mr Shaw attaches great importance to this fact, the experimental particulars of which are carefully detailed in his paper. It had, in truth, been objected to Mr Shaw's former experiments, that by a forced alliance between the parr and salmon, he had not proved their identity, but had merely succeeded in producing a *hybrid*. The brood, however, in no way differs from other young salmon in the parr stage, so far as external characters are concern-

ed; and their procreative powers afford an additional confirmation of their being identical. Mr Shaw moreover observes, that, if the parr were a distinct species, the results of their attendance on the female salmon would produce universal confusion among the migratory inhabitants of rivers, "from the circumstance of the male parrs, in a breeding state, occupying in great numbers the very centre of the salmon spawning-bed; while the female salmon herself is, at the same instant, pouring thousands of her ova into the very spot where they are thus genially congregated."

Mr Shaw's experiments were conducted with great care; have been frequently verified by repetition; and have always been followed by the same results. He concludes his paper by stating, that so far as his own observations extend, the trough, or special spawning-bed, is excavated by the female alone, and by means of a peculiar action of the tail. The belief hitherto entertained has been, that she was greatly aided in this removal and replacing of the gravel by the snout and under jaw of the male. The process of laying usually occupies three or four days. He found in the course of his experiments, that the death of the female did not (at least within a short time) destroy the vitality of the spawn, or its receptive power of fecundation. He impregnated ova from the body of a fish which had been dead for nearly two hours, with the milt of a living parr, and vivification followed in the usual course.

[In reference to the preceding abstract of Mr Shaw's interesting and important observations on salmon fry, we shall here merely remind the reader, that this ingenious inquirer's earlier experiments were first communicated to the public through the medium of our own Journal. See *Edin. New Phil. Journ.* for July 1836, vol. xxi p. 99, and for January 1838, vol. xxiv. p. 165. We shall present a consecutive view of the whole subject (with an illustrative plate), in our next number. Meanwhile, we may state in addition to the points glanced at in the above abstract, that as it had been objected to Mr Shaw's practice in reference to the mechanical mode of impregnation, that the generative influence might have been in some other way effected than through the medium of the parr, he therefore took every means to prove the truthful results of his experiments, by varying in some measure their conditions. Thus, in two instances, he took a portion of the ova from a female salmon, and placed it, *without impregnation*, in a stream of pure water. The result was as he anticipated: up to the termination of the general hatching season no appearance of vitality was ever manifested. The females were the same individuals with which the parrs were made to spawn, as above mentioned, but to avoid contact, the unimpregnated lots were in each case taken first, and removed to a distance. The trough or spawning-bed, Mr Shaw observes, is formed by the female fish without the assistance of the male, and not by means of the snout, as usually supposed, but of the caudal extremity. This last observation, we may note, had been previously made by Mr Potts of Berwick, in the course of his correspondence with Pennant. —EDIT. PHIL. JOURN.]

2. On the use of the word "Temperature" in the Analytical Theory of Heat. By Professor Kelland.

A third communication by Professor Forbes, "On the Influence of the Mechanical Texture of Screens on the imme-

diate transmission of Radiated Heat," was placed in the hands of the Secretary, and the reading of it postponed till next meeting.

1840, *January* 6.—Sir T. M. Brisbane, Bart. President, in the Chair. The following Communications were read:—

1. On the Effect of the Mechanical Texture of Screens on the immediate Transmission of Radiant Heat. By Professor Forbes.

2. On the Functions of the Roots of the Spinal Nerves in corroboration of former observations, proving that the anterior roots are nerves of motion, and the posterior nerves of sensation. By Sir Charles Bell, K.H.

The instance produced was a case which had occurred in the Infirmary, in which the anterior roots of the nerves arising from the *cauda equina* were engaged in a tumour, and the consequence during life, was the loss of muscular power, sensibility remaining unaffected.

January 20.—Right Hon. Lord Greenock, V. P. in the Chair. The following Communications were read:—

1. On General Differentiation. Part II. By Professor Kelland.

2. On the Means employed to prevent Forgeries of Bank Notes, Bills of Exchange, Cheques, &c. By Dr Traill.

"The author prefaced his remarks by a short history of Bills of Exchange, and other kinds of paper currency; in which he pointed out the economy and convenience of a well regulated *paper circulation*.

"The means usually employed to diminish the facilities to forgery are—

"1. Colouring the paper, or the pulp of which it is manufactured, by various vegetable and mineral substances, either singly or combined. The best attempts of this sort have been imitated by a clever hand and a pencil.

"2. Certain *water-marks* in the paper of the note or bill, as that employed by the Bank of England. A good imitation of this would perhaps require the connivance of a paper-maker; but successful imitations of *water-marks* have been made by gum and a pencil, or by pale tints of China ink to imitate a worn note.

"3. Printing notes in different colours, from two or more wooden blocks, increases the difficulty of forging; instances of this were given in the coarse Excise paper stamp, and in the more elegant L.1 note formerly issued by the Bank of Ireland.

"4. The excellence of the engraving—a well executed imitation would require the aid of an able artist; and as engraving on copper and on wood produce these effects by *opposite* means, if designs from both were used on each note, the difficulty of engaging *two* eminent artists in such a nefarious trade would enhance the obstacles to successful forgery. The tracing on copper by an engine any series of curves, may easily be imitated by what is called engine-turning; but the most perfect specimen of this sort of engraving is that devised by Mr Perkins.

"5. Numbering each note has been adopted by all banks, and some number them both with impressions from copper and from wood. These have all been successfully imitated; yet this numbering often has led to the detection of forgery. The numbering-engine, used by the Bank of Ireland, is the most perfect which the author has seen, combining speed with accuracy, and registering its own work with unerring precision.

"6. The signatures of some of the officers of the bank have usually been added to the note, from the known difficulty of exactly imitating a handwriting.

"7. Frauds on bills and cheques have been committed by deletion of the common ink in which the amount of the bill has been written—the substitution of a larger sum. The author recommends the use of an ink, not effaced by chemical agents, without damaging the paper. For this purpose, he conceives that a carbonaceous ink, submitted by him to this Society some time ago, is well suited; especially when used on the softer kinds of paper, such as that made by the engine now used for making paper in one continuous sheet."

February 3.—Sir T. M. Brisbane, Bart., President, in the Chair. The following papers were read:—

1. Observations on the Influence of various circumstances in Vegetation in modifying the Physiological Actions of Plants. By Dr Christison.

"The author commenced with some remarks on the various causes by which the actions of plants and of their products on the animal body may be modified, and on the great vagueness and uncertainty of the information at present possessed in regard to the influence of those causes which seem to arise in peculiar circumstances of vegetation, more especially climate, weather, soil, and the progress of vegetation. He then stated the sources of information on these points, namely, the curative or therapeutic action of drugs on man,—their effects on the healthy functions both of man and animals, either as medicines or as poisons,—their sensible qualities,—and their chemical analysis; and he assigned reasons for discarding the first of these from the inquiry, and for trusting, in a great measure, to the criterions derived from sensible qualities,—from

the effects of poisons on the lower animals,—and from chemical analysis.

" The remaining part of the present paper was confined chiefly to the influence of the Progress of Vegetation on the activity of plants. Doubts were thrown by the results of his investigations on most of the current doctrines on this head; but the present state of the inquiry did not lead to any general inferences being drawn with confidence.

" An extended statement was made upon the influence of the progress of vegetation upon many of the active species of the natural family *Ranunculaceæ*. It was stated, that, in the acrid species of the genera Ranunculus, Anemone, and Clematis, the acridity, which is the same throughout them all in quality, is possessed in nearly equal activity by the leaves, from an early period in the spring until they are about to decay; but that it exists in the germens only while they are green, and disappears there entirely as the seeds ripen. In the acrid species of *Aconitum*, the acridity of the leaves, on the contrary, continues only until the seeds begin to form, and then gradually, but quickly, disappears as they ripen, while the seeds acquire precisely the same peculiar kind of acridity. The narcotic properties of the leaves, however, do not undergo the same singular change, but continue undiminished after the seeds are mostly ripe, and probably, indeed, as long as the leaves themselves retain their freshness. The acridity of the genus *Helleborus* is probably governed by circumstances different from any of those already mentioned; but the experiments already made are insufficient to point out the true rule.—In the course of these observations many remarks were also made on the nature of the acridity possessed by the different species, upon which incorrect ideas at present very generally prevail; several material corrections were also suggested as to the general opinions respecting the influence of heat, desiccation, and time, upon their acridity; and a short allusion was made to the properties of a remarkably crystalline principle which the author discovered in one of the species of Ranunculus, and which appeared to him to be the ingredient upon which the activity of that genus depends.

" The author next entered into some details regarding the influence of the progress of vegetation on narcotic plants, and commenced with the natural family *Amygdaleæ*, the leaves of several of which are eminently poisonous, in consequence of containing, or producing, when bruised, a hydrocyanated essential oil. He shewed that this oil abounds most in the leaves of the cherry-laurel (*Cerasus laurocerasus*), when they are young and undeveloped, and that it goes on diminishing gradually in proportion to their weight, as they increase in age and vigour, until the commencement of their second season, when the old leaves, though plump and luxuriant, do not contain above an eighth or tenth of what they contained in the infant state, or of what is contained in the young undeveloped leaves of the same period. This is a complete reversal of the generally-admitted law in respect of the formation of volatile oils in leaves.

"The consideration of this fact led to some statements upon the mode and form in which some essential oils, and other active principles, exist in the leaves of plants; and the conclusion was drawn, that in all probability many active principles, which are separated from plants by simple processes, do not exist ready formed in the leaves; but, as in the familiar case of the mustard-seed and bitter almond, are only developed when the structure of the leaves is broken up, and principles of a different kind, secreted in distinct cells, are brought in contact with one another, or with water.

"The remaining departments of the investigation were postponed; but further observations were promised, upon the influence of the progress of vegetation on the Solanaceous and Umbelliferous plants; and likewise on the effects of soil and climate."

2. A Specimen of *Clavagella Balanorum* (Scacchi), presented by Sir Thomas Brisbane, Bart., President of the Society, was laid on the table, and described by Dr Traill.

Proceedings of the Wernerian Natural History Society.

(Continued from p. 200.)

Dec. 7. 1839.—Dr R. K. Greville, Vice-President, in the Chair. Mr R. Hay Cunningham read a paper on the relations of *Granite* to *Stratified* Masses, and its analogy to *Trap*.

After a short introduction, in which the author of this paper stated the commonly received opinion in regard to granite, this being one which viewed it as of an igneous origin, and that it had produced violent changes in the stratal position of rocks, he remarked, that it was the latter statement only which he was then to examine. The principal phenomena which had produced the belief that granite had effected motion, was affirmed to be, first, "That granite, as trap, occurred in veins invading strata;" and, secondly, "That, as in the case of trap rocks, angular masses of contiguous strata appeared imbedded in granite." Having made these remarks, Mr Cunningham said, that he would first examine the position of strata on the great scale to granite mountains, and subsequently consider the more minute connexion of such rocks with each other. In regard to the position of strata in their great features, the author observed, that he had not found these to exhibit more evident marks of derangement when near granite than when at a distance from it. He enumerated many granitic districts in Scotland which he had examined (among which were those of the Grampians, of Galloway, Dumfriesshire, Banffshire, Aberdeen, Arran, and the Hebrides) with the view of satisfying himself

as to how far the popular notions agreed with natural appearances, and said, that from this he had found that the phenomena detected by him in the field gave quite the reverse of support to the sentiments of the greater number of British and many Continental observers. He had not noticed what is called an anticlinal arrangement in the strata around granite masses, but, on the contrary, had observed that these probably never derange that SE. dip which is so universally held by the transition or grey-wacke group, and the more ancient or gneiss and mica-slate systems of Scotland. It was stated, also, that what are styled "contortions," are as abundant at a distance from granite, as in its immediate vicinity. Mr Cunningham having concluded his notice of the positional relation of strata to granite, next proceeded to examine the connexion of granite veins with strata, defining that what he meant by granite vein, was a "granite mass, one extremity of which was found to originate in a principal mass of granite rock." Granite was stated to have been considered by most geologists as affording strong arguments in favour of its having been so produced as to effect motion among strata,—the author, however, said, that he could not agree with these observers in their opinions concerning granite; he addueed numerous reasons of dissent, among which were, 1st, That, in almost every instance, the integrant laminæ of a stratum agree so exactly with each other on both sides of a granite vein, that if the intervening granite could be removed they would be in contact without exhibiting the slightest dislocation or shift. 2d, That granite veins, when occurring in apparently bent strata, are often disposed among their laminæ or lines of stratifications, so as to conform to their most intricate flexures. These associations were viewed as shewing that the relations of strata to a great granite region, were the same as those which existed between a granite vein and the position of strata or of their composing laminæ. Numerous localities were cited where the sceptical in regard to these facts might test them; the Needle's-eye, in Dumfriesshire—the Banks of Loch Doon—the Windy Shoulder, near New Galloway; Arran, and Sutherlandshire, were especially pointed out as being localities where the detailed phenomena were exhibited; the reason why these had been selected was stated to be, that they had generally been considered points where the Plutonic formation of granite was well supported.

3d, The author said, that some might view what he had described as granite veins conforming to contorted strata as mere alternating layers of granitic gneiss, but observed, that it was impossible to view them in such a light, for he had traced them into a larger granite mass, and had found also several of the granite conforming veins uniting to each other by ramifications from them, which branches or ramifications crossed the strata at an angle.

Mr Cunningham then adverted to the "*so-called fragments*" of strata which occur in granite mountains and granite veins, and stated, that whether these were to be viewed as fragments or not, they seemed to indicate that they had not been subjected to violent motion, for the reason

that, whatever their size might be, the position of their strata or laminæ was the same as that which the strata of the same rock held when at equal distances from the granite. Localities being mentioned where these phenomena were well exhibited, Mr Cunningham stated, that he would now review another class of granite masses, and which, he said, might perhaps be denominated "imbedded granite veins," from the circumstance, 1st, that these occur as layers alternating with strata, but thinning out at both extremities; 2d, as masses crossing strata at all angles; 3d, as masses in some parts of their extent running parallel with the strata, and also crossing them; while, 4thly, they form bodies of circular or elliptical shapes, which, in some instances, are contained within *one* stratum, while in others they are inclosed among several. Farther, it was affirmed that all the forms of these imbedded granite masses, emitted, as true veins do into strata, numerous small branches, which never change the position of the strata in which they occur, or of their laminæ, and also that if masses of the strata occurred imbedded in these concretions, that such included portions had a laminar arrangement of the same position as that which was held by the adjoining strata, it mattered not whether that were rectilinear or convoluted. Mr Cunningham also observed, that these imbedded granite masses varied in shape to a great extent, that one might exist as a layer for many 100 feet, and at last swell out into a most irregularly round shaped body, which might perhaps send out another small ramification, again to finish in another large mass. In size, the imbedded granite concretions were said to be from that perhaps in which a granite concretion might be produced by the aggregations of a few crystals, to one of an immense magnitude. In regard to the relations of the smallest concretions to strata, it was stated that these were the same as those of the largest, inasmuch as they never disturbed the laminæ in which they occurred, and, in conclusion, it was mentioned that the only difference which existed between a minute granite concretion and a granite mountain was in their relative magnitudes. After mentioning that granite veins and granite concretions appeared to be only varieties of each other, it was affirmed to be supported by these circumstances, viz. that, between the great bulk of a granite concretion in strata and a mountain mass or true vein of granite, there has been found to occur, either a series of minute detached concretions running more or less onwards to the mountain mass, or a series of concretions, some of which were joined to each other by small processes, or a granite concretion, often of an immense size, was found to be conjoined with the chief body of the rock, through means of a granite layer, perhaps not exceeding a few lines in length, and the position of which holds no certain relation to that of the strata. The author next proceeded to examine the relations of granite veins to granite veins, and of granite concretions to granite concretions. The general opinion that of two veins, the crossing vein must be the last formed, was noticed. When comparing this preva-

lent opinion with granite veins, it was seen to be in many cases unsubstantiated, and as a proof of this, instances were brought from many granite districts, where granite veins passed through each other, in some cases to all appearance causing dislocations in these, while in others they did not; whether or not, however, there was the least resemblance of a shift in these veins, they nevertheless all occurred in perfectly unshifted strata and laminæ. Mr Cunningham remarked, however, that, although such a phenomenon as that might render the shifted nature of granitic veins very probable, the reverse appearance which he had often seen, viz. the fact of unshifting veins in shifted strata and laminæ, rendered it still more probable that the whole of these indications of motion, whether in the strata or in the granite, are of an entirely deceptive nature. In regard to the relations of what are called contemporaneous veins of granite to each other, it was said, that the very same phenomena presented themselves, and that such granite veins sometimes cross others and shift them, but that all are so inclosed in granite, that in some instances when it is large granular, crystals of felspar and mica were detected, which, although separated by these veins, were still completely undisturbed. As in instances that had before been explained, there was then a mass, to all appearance shifted, enclosed in another evidently unshifted. Concerning the relations of granite concretions to each other, it was from many instances shewn that these were of the same kind as the relations of granite veins to each other, and to the strata they occur in. When, on this part of the subject, the author brought forward in support of his opinions in regard to granite, a case from Sutherlandshire, in which a large concretion traversed strata and shifted another great concretion, and said that, what was remarkable, a portion of the shifted concretion sent veins into what in plutonic language was to be viewed as the shifting agent. Mr Cunningham then proceeded to examine the more minute relations of one crystal to another, and from this shewed that the appearances only differed from the relations of strata to granite on the great scale, and of granite veins and concretions to strata in the circumstance, that the relations of crystals to each other were on an almost microscopic scale. The connections of granite being thus described, the author then proceeded to examine their resemblance to phenomena attending the various members of augitic or trap series, it was shewn that that group, though it is most probably of an igneous origin, had still, through some unknown process, so connected itself with stratified masses, as to cross their horizontal beds without deranging them. Mr Cunningham, in proof of the fact that even trap had sometimes not effected motions in rocks, or at all events left no signs of it, described two instances in which he had found fossil organic bodies to be crossed by veins without disturbing them. One instance noticed was from the cliffs of Camustranivig in Skye, where a large ammonite was traversed by a trap vein; another was from Mull, where several gryphæa incurvæ were similarly crossed. In

neither instance was there the slightest derangement of the fossils visible. Having illustrated the details we have reported, Mr Cunningham concluded by advising geologists to pay greater attention than was generally done to the mineralogy of rocks. The paper was accompanied by many large sections descriptive of the details which were entered into.

There were laid before the meeting tables of several years' meteorological observations made at Fort Vancouver, and for a whole year among the Sandwich Islands, by the late Dr Meredith Gairdner.

December 21.—Professor Jameson, P., in the Chair. The Secretary read a communication on Professor Ehrenberg's discoveries respecting the Microscopic Animals which form the chief component parts of Chalk and some of its associated rocks. Dr Greville read a letter from Mr Shuttleworth of Berne, stating, that some of the organisms supposed by Professor Ehrenberg to be of an animal nature had more probably a vegetable origin. The President exhibited a large specimen of green fluor-spar which had been observed in a new locality in Banffshire, (the "Three Burns," between Gaulrig and Little Fergie, River Aven), by Mr Hay Cunningham. Mr Stevenson mentioned, that, up to the 1st December, no snow lay upon the mountains of the Outer Hebrides, which, in ordinary seasons, are clothed with snow in the end of October or beginning of November; but that wild swans were abundant in the lochs, indicating a severe winter in Iceland and Faroe.

January 11. 1840.—Dr T. S. Traill, V. P., in the Chair. Professor Jameson read various extracts from communications received from his nephew, Mr W. Jameson of the Bengal Medical Service, on the Natural History, and more especially the Geology, of the portion of the Himmaleh range near Simla. Mr Sligo exhibited a large slab of the Mussel-band Ironstone, lying immediately over the coal at Airdrie, and containing some interesting remains of Ganoid fishes.

January 25.—Dr R. K. Greville, V. P., in the Chair. Professor Jameson read extracts from Lieutenant Newbold's description of the valley of Sondur in Southern India, and of the mountains which surround it. Mr Edward Forbes read an account of the Entozoæ of the Cydippe pileus, shewing that, besides a Filaria, there is a curious parasitic animal, which was first detected by Major Playfair of St Andrews, and has been called by Mr Forbes Tetrastoma Playfairii. The Reverend Mr Wallace's Meteorological Table for 1839, and Lord Gray's, were laid on the table. Professor Jameson read an analysis of a Clay eaten by the Negroes in the West Indies. Dr Neill, the Secretary, then read Mr Thomas Fraser's account of the Geology of the western part of Morven. Professor Jameson took this occasion to state his views in regard to the so-called stratified structure of primitive rocks, which he, in conformity with his papers in the Philosophical Journal, and others read before the Society at different times, is still disposed to consider as an effect of crystalliza-

tion; and, therefore, strictly speaking, a tabular, not a true stratified structure.

February 8.—Dr Charles Anderson, formerly V. P., in the Chair. Dr Paterson read a paper on the manner in which animals, and more especially fishes, have been deposited or inclosed in secondary rocks. The Secretary read Mr Thomas Geadell of Preston's account of the structure and habits of the common Mole, *Talpa Europæa*, founded on the experience and observations of many years. A letter was read from Mr H. M. K. Esmark (from Brevig in Norway), accompanying a specimen of the rare mineral Thorite.

February 22.—Dr R. K. Greville, V. P., in the chair. The address to the Queen on the occasion of her marriage, agreed upon by the Council on the 15th, was read and approved of. Professor Jameson gave an account of a new mineral, first noticed by Lord Greenock, which he proposes to call Cadmium Blende, or Greenockite. (See this No. of the Journal, p. 390). Mr Hay Cunningham, read an essay on the Geognosy of Arran and Lamlash, which was illustrated by numerous sectional drawings and specimens. The following are the inferences which he has drawn from his investigations:—

1*st*, That the oldest stratified rocks of Arran consist of an alternating system of clay-slate, mica-slate, and greywacke.

2*d*, That a series of secondary strata rest upon these unconformably.

3*d*, That the series mentioned is the independent coal-formation of Werner.

4*th*, That an examination of the secondary strata of Arran, and a comparison of these strata with the secondary strata of the south of Scotland, demonstrate the incorrectness of the opinions of Professor Sedgwick in regard to them, as these opinions are expressed in a paper published in the Trans. of the Geological Society.

5*th*, That the granite of Arran, though altering the structure of the strata, has not *positionally* altered them.

6*th*, That the stratified rocks of Arran and Lamlash are traversed by rocks of porphyry and greenstone.

7*th*, That an examination of the unstratified rocks of Arran allows us to fix their relative periods of formations, the result of such an examination being, that the granite is the oldest unstratified rock, the porphyry next in point of age; while the trap, of which there are two sets, one an older and another a newer, constitutes, with the pitchstone, the newest rock of the island.

8*th*, That the islands of Arran and Lamlash have been swept by at least one large body of water.

Dr Traill exhibited a specimen of native gold in quartz, found by himself at Wanlockhead, in the year 1802; and also some other rare British minerals.

March 7.—Dr R. K. Greville, V.P., in the chair. Dr Robert Paterson gave an account of a nondescript fossil vegetable observed by him in the shale near Newhaven. It consists essentially of a cylindrical head or catkin, with two divisions. To this is attached a striated stem, with occasional enlargements on it. It approaches, in general appearance, to several living genera of plants, such as Typha, Equisetum, Piperomia, and Pothos; to the latter of which, however, it seems most closely allied. The genus Pothos is now entirely a tropical one; which circumstance affords additional evidence of the great similarity which existed between the plants which must have so luxuriantly flourished in this country at the period when the coal strata were deposited, and those of tropical climates at the present day. This fossil seems to be a rare one, only a single specimen of it having hitherto been found.

At the same meeting, Mr Robert Bald, mining-engineer, read an account of the Black Band Ironstone of Scotland, now in such great demand for the production of cast iron. He stated that, less than forty years ago, this rich stratum of ironstone, though repeatedly passed through in working for coal, was not recognised as being of any value, but was thrown on the rubbish-heap as worthless. About thirty-four years ago, however, Mr David Mushet, while crossing on foot a shallow part of the river Clyde, took particular notice of this layer of ironstone: he regarded it as being of a peculiar aspect, very different from any of the other argillaceous ironstones, being combined with a large portion of carbonaceous matter. It was thereafter wrought extensively and profitably by the late Mr William Dixon, of the Calder Iron-Works, for more than fifteen years before any one thought of searching for a similar ironstone; in short, it was considered as a fortuitous matter, probably confined to a very limited locality at the head of the Monkland Canal. By degrees, however, it began to be sought after; and it has been ascertained to be a stratum of very general occurrence in the coal-formation of Scotland. Scarce a week passes without a new discovery of it; so much so, that it has already been found (chiefly by Mr Geddes, partner of Mr Bald), in the counties of Lanark, Ayr, Fife, Clackmannan, Dumbarton, Stirling, and Mid-Lothian; it occurs in our own neighbourhood, at the Dryden and Gilmerton Collieries, along with the edge coals. This Black Band Ironstone possesses the following remarkable characters: it has carbonaceous matter enough in its composition for calcination without the aid of coals. When calcined, it has all the appearance of iron, and is very heavy; indeed, lumps of it have been taken from the heap, and immediately, in a smith's forge, made into a horse-shoe. Mr Bald stated, that with this rich ironstone, aided by the hot-blast, those furnaces which used to produce thirty-six tons of iron a-week, now readily afford 100 tons. He added, that, a few years ago, there were only nine blast furnaces in Scotland; but now there are forty-two, and more in progress. To give an idea of the value of the Black Band, he mentioned, that Sir William Alexander of Airdrie, had let to an iron company a limited extent of this mi-

neral, at the enormous rent of L.12,000 per annum. In conclusion, Mr Bald feelingly regretted that justice had not hitherto been done to the merits of Mr Mushet, who made the discovery of this ironstone; for this meritorious individual remains almost unknown, and certainly unacknowledged by the iron-masters and mine-owners of Scotland.

Proceedings of the Microscopical Society.

The Microscopical Society of London, held their first Meeting on Wednesday, January 29. at the Horticultural Society's Rooms, No. 21 Regent Street. The meeting was attended by upwards of a hundred members and visitors.

The President, Professor Owen, announced that, since the provisional meeting on the 20th of December for the purpose of forming the Society, the number of members had increased to one hundred and ten, and a further addition of twenty-nine names was announced in the course of the evening, making a total of one hundred and thirty-nine original members of the Society, it having been determined, that those who joined the Society on or before the first night of meeting should be considered original members.

Mr Owen communicated a paper on the application of Microscopic examinations of the structure of teeth to the determination of fossil remains. After alluding to the essential service rendered by the microscope to the chemist, mineralogist, and vegetable physiologist, he proceeded to offer a few examples of the utility of the microscope to the geologist, when applied to the investigation of the structure of fossilized teeth.

The first example adduced was that of the Saurocephalus, an American fossil animal which had been referred to the class of reptiles. After pointing out the distinctive characters of the microscopic texture of the teeth in reptiles and fishes, it was shewn that the Saurocephalus, according to this test, unquestionably belonged to the latter class, and that it most closely resembled Sphyræna among recent fishes in its dental structure.

The second instance was the Basilosaurus of Dr Harlan, which had been referred to the class Reptilia; and the double-fanged structure of its teeth had, on the strength of its supposed Saurian affinities, been adduced to weaken the arguments advanced in favour of the mammiferous nature of certain fossils from the Stonesfield oolite. Mr Owen, after describing the microscopic character of the teeth of the Basilosaurus, shewed that it deviated from the Saurian structure in this respect as widely as the Saurocephalus, but that the modification of its dental structure resembled most closely that of the cachalot and herbivorous Cetacea. Lastly, Mr Owen alluded to the difference in the views entertained by Cuvier and M. de Blainville, as to the affinities of the megatherium, which was referred by the one to the family of the Sloths, and by the other to that of

the Armadillos; after explaining the well-marked differences in the microscopic characters of the dental structure in these two families of the so-called Edentata, Mr Owen proceeded to describe the structure of the teeth of the megatherium, and to shew that in its close resemblance to the dental structure of the sloths, it confirmed the views of the great founder of the science of fossil remains. This paper was accompanied by a number of very beautiful illustrative drawings, exhibiting the minute structure of the teeth of the animals referred to.

Mr Jackson then read a short paper, drawing the attention of the Society to a mode of mounting the compound microscope, which differs in some particulars from the methods generally adopted. The principal object to be kept in view in the construction of the instrument, is the prevention of those accidental vibrations which so much interfere with microscopic examinations, especially in the neighbourhood of crowded thorough-fares. This object is effected by connecting together the body and stage of the instrument in such a manner, that whatever vibrations are communicated to the one shall be equally communicated to the other. In the instrument of Mr Jackson this principle has been carried further than has hitherto been effected; and it also affords improved facilities for minute adjustments, and the accurate admeasurement of microscopic objects.

A discussion ensued on the subject of Mr Jackson's paper, and also on the best methods of measuring microscopic objects, and the greater difficulties encountered in ascertaining the antero-posterior diameters of minute bodies, as compared with the facilities whith we possess of obtaining lateral measurements. The meeting then resolved itself into a conversazione, during which a number of interesting objects were exhibited by individual members, many of whom had their microscopes upon the table.

The meeting adjourned at 11 o'clock.

Wednesday, February 19. 1840, R. H. Solly, Esq. in the Chair.—A paper was read by Mr Quekett, on the development of the vascular tissue of plants, in which it was shewn that the membranous tube of vessels originated from a cytoblast in a manner similar to that described by Schleiden in the formation of cells,* from which at first it is difficult to recognise them; but in a short time they assume a very elongated form, and the cytoblast disappears. Before the fibre is deposited, the contents, which are gelatinous, are crowded with numerous most minute granules, which possess the motion known as that of "active molecules," and after a short time when they have become a little enlarged, they adhere to the inner surface of the tube containing them in a different manner for each vessel, so that the several varieties of vascular tissue are not degenerations of each other, but are each constructed originally on the plan they are always observed to present to the eye.

* See Taylor's Scientific Memoirs, vol. ii. p. 291.

It had been conjectured by Schleiden that a current existed between the gelatinous contents of the cell and its walls, which preceded the formation of a fibre, and gave the direction it afterwards took; this was refuted by shewing, that the granules became separately attached to the inside of the vessel, at a little distance from each other, beginning first at one end and proceeding to the opposite; the fibre elongating like a root, by the materials of growth being always added to the point. The granules so attached, becoming nourished by the contents of the vessel, and the spaces between them, are in a short time obliterated by the fibre acquiring a defined border which completes its development.

This act is the one observed in the formation of the fibre of all vessels, but the arrangement of the granules differs so as to constitute the several varieties. In the annular vessel the granules attach themselves horizontally, forming rings. In the spiral they become inclined, and by continuing this direction around the interior of the membranous tube, the peculiar character of the vessel is obtained. In the reticulated, each division or branch of the fibre or granule becomes enlarged in the line, and forms the starting-place for the fresh direction of the fibre. In the dotted and scalariform vessels, the fibres become so reticulated as to have portions of the outer membrane of the vessel without any deposit within; and this spot so left, constitutes the dot or linear marking seen in these vessels.

This dot is plain in all such vessels, excepting those found in woody exogens, where it possesses (from a slight difference in structure) a central mark analogous to that on the woody tissue of coniferous plants with which Mr Quekett considers it identical, but only of a smaller size. The paper was illustrated with numerous diagrams, which gave representations of the successive stages of the minute process Mr Quekett had observed.

SCIENTIFIC INTELLIGENCE, IN GEOLOGY, MINERALOGY, &c.

1. *Lantan, a new Metal.*—Mosander has detected, in the cerite of Bastnäs, a new metal, which he names *Lantan.** The oxide of cerium, obtained by the usual process from the cerite, contains two-fifths of its weight of the oxide of the new metal, which alters the properties of cerium very little, and was thus as it were concealed in it. Hence Mosander names the new metal *Lantan*. Kersten of Freyberg has found this new metal asso-

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* Lantan, from λανθανειν, to be concealed.

ciated with phosphoric acid, thorina, oxide of cerium, oxide of tin, &c., in the *monazite* of Miask.

2. *Chemical Composition of Datolite and Botryolite.*—It results from the analyses of Carl Rammelsberg (Poggend. No. 5, 1839, p. 169), that datolite and botryolite are distinguished from each other by their proportion of water of composition,—botryolite containing double the quantity of water of datolite.

3. *Andalusite and Chiastolite varieties of the same Species.*—From the experiments of R. Bunsen of Cassel, in Poggendorff's Annalen, No. 5, for 1839, p. 186, it appears that the chief ingredients of andalusite and chiastolite are as follows:—*Andalusite*, silica, 40.66; alumina, 59.34 = 100.00. *Chiastolite*, silica, 40.03; alumina, 59.97 = 100.00. Hence it appears that, on chemical principles, andalusite and chiastolite are identical, or only varieties of the same species.

4. *Chemical Composition of Mikrolin.*—The mikrolin is proposed by Breithaupt as a distinct species, and separated from felspar. The separation rests chiefly on small differences in the angles of the crystals. As the determination of the true nature of this mineral was also of importance in a geological point of view, M. J. v. Ewreinoff, captain in the Russian Engineers, analysed it under the eye of G. Rose, and obtained the following result, which shews that this mineral agrees with felspar in chemical composition, with exception of the small quantity of lime, which has taken the place of a portion of the potash:—Silica, 65.761; alumina, 18.308; lime, 1.200; potash, 14.060; soda, magnesia, and oxide of iron, a trace, = 99.329. —*Poggendorff's Annal.* No. 5, p. 196, 1839.

5. *Chemical Composition of Monazite.*—This is one of the many new mineral species added to the system by Breithaupt (in 1829). As it cannot well be compared with any known species, and appears in some degree isolated in the system, he named it *Monazite*. Its peculiar chemical constitution also shews the name to be well chosen. Kersten of Freyberg (*vide* Poggendorff, No. 6, 1839, p. 385,) found its composition to be as follows:—oxide of cerium, 26.00; oxide of lantan, 23.40; thorina, 17.95; oxide of tin, 2.10; oxide of manganese, 1.86; lime, 1.68; titanic acid and potash, a trace; phosphoric acid, 28.50 = 101.49.

6. *Mineralogical Optical Notice by Babinet.*—Hyalite cut in any direction into plates with parallel faces, and brought between two crossed tourmalines, depolarises light completely. By this character it is distinguished from milkopal and Mexican fireopal.

7. *Malthacite, a new Mineral Species.*—Malthacite (from μαλθαχος, fat, which it resembles) was found by Törmer among decayed blocks of basalt near Steindörfel, between Lobau and Baudissin. Colour white or yellowish; lustre glistening and resinous; translucent; occurs in thin splinters, rarely massive; fracture uneven, conchoidal; easily frangible; feels greasy; falls into pieces in water. According to O. Meissner it is composed of silica, 50.2; alumina, 10.7; lime, 0.2; oxide of iron, 3.1; water, 35.8. Its formula nearly FS^4+3AS^4+5 Aq. Decrepitates before the blowpipe, affords water, but does not melt.

8. *Serpentine Crystals of Snarum in Norway.*—Quenstedt is of opinion that the mineral resembling steatite, found at Snarum in crystals, imbedded in serpentine, and whose form is identical with that of olivine or chrysolite, is decomposed olivine. Tamnau (in Poggendorff's Annalen, 42, p. 462,) considers this opinion as very improbable. He suspects that the after or supposititious crystals are rather formed by the decaying olivine being carried away, and the hollows thus left being filled up with the same substance which forms the surrounding rock, namely the Serpentine.

9. *Opaline-Allophane.*—Schrötter (Poggend. Annalen, 40, p. 380) has analyzed a mineral found at Döllenberg, near Frauenstein, in the Brucker Circle, which he names opaline-allophane, and which occurs in nests between mountain-limestone and clay-slate. Its characters are as follows:—Colour bright emerald-green, passing by degrees into bright brown. Exhibits no determinate structure or cleavage; lustre vitreous; semitransparent; affords a white streak—sp. gr.=1.985 to 2.016; before the blowpipe is infusible, either alone or with soda, and consists of silica, 11.95; alumina, 46.30; oxide of iron, 2.95; lime, 1.30; sulphuric acid, 0.78; oxide of copper, 0.25; water, 36.20 = 99.73. Hence the formula is

$A^2SAq^4 + 2\,AAq.$, or $A^4S + 6\,Aq$, leaving out the other ingredients which are viewed as accidental.

10. *Davidsonite.*—This so-called new species found in the granite of Aberdeen, turns out to be a mere variety of beryl, as is shewn by an examination of its mineralogical characters and chemical composition.—*(Poggend.)*

11. *Perikline.*—Breithaupt gives the name Perikline to a mineral belonging to the natroniferous felspars, because it possesses a lower specific gravity, and exhibits some difference of magnitude in the angles of its crystals from albite. This mineral, from St Gotthardt, has been analyzed by Thaulow, who finds its composition to be as follows:—Siliceous acid, 69.00; Alumina, 19.43; Natron, 11.47; Lime, 0.20 = 100.10. This affords the formula, $NS^3 + 3\,AS^3$ which is that of albite. G. Rose has examined the angle, and finds it somewhat different from that of albite, which, however, is nothing more than can be deduced from the circumstance that the faces of Perikline are generally more or less streaked and arched or convex. As to the specific gravity, that of albite is = 2.61 to 2.63; and Perikline from the Tyrol and St Gotthardt = 2.437 to 2.457. But when the Perikline was pulverised, its specific gravity was from = 2.637 to 2.647. The specific gravity of pulverised albite was not determined. Hence, Berzelius considers Perikline and albite as belonging to the same species.

12. *Chloritoide.* — This mineral, formerly named chlorite spar, now Chloritoide by G. Rose, was discovered in the marble quarry of Mamorskoi. Bonnsdorf, as stated in G. Rose's Travels to the Altai and the Caspian Sea, found that it is composed of siliceous acid 27.48; magnesia, 4.29; oxide of iron, 27.05; oxide of manganese, .030; alumina, 35.57; water, 6.95. Hence, according to Bonnsdorf, its formula = $\frac{f}{mg}\,S + A^2\,S + 3\,Aq$; which differs from that given by Erdmann.

13. *Scale for the Fusibility of Minerals.*—Von Kobell has proposed a scale for the fusibility of minerals, as a contribution to the distinctive characters obtained by the blowpipe. He selects the following minerals for his scale, of which the first is the most easily fused, the last the most infusible. 1. Grey antimony or sulphuret of antimony; 2. Natrolite; 3. Almandine; 4. Actinolite; 5. Adularia; 6. Diallage. We pre-

pare fine splinters of these minerals and compare them with similar splinters of the mineral whose fusibility is to be ascertained. These are expressed by the six numbers, with the addition of a decimal number, which expresses nearly how much more difficultly fusible the mineral appears to be, than the corresponding number of the scale.

14. *Recent Deposits of Salt.*—M. Engelhard informs us that mining implements have been discovered in the salt-mines of Hallein, and in such a position, in regard to the beds of rock-salt at present worked, as to lead to the conclusion, that deposits of salt have taken place since the commencement of the working of these mines, deposits formed by the action of water on the previously existing beds of rock-salt. This fact is interesting in many respects, and affords a warning to geologists to be careful to distinguish between original and ancient deposits, and those of a very recent date formed by the action of water and air on ancient formations.

15. *Height of the Coast-line of Fuci.*—In the account of the results of the French Northern Scientific Expedition, it is stated, that the determination of the mean level of the sea has permitted the measurement, at a great many points, of the line of marine algae (Fucus vesiculosus); a line which is horizontal and well defined, forming for the observer a very good mark along the whole coast of Finmark. At the same time a verification has been obtained of the continuity of the lines and terraces which indicate what was the ancient level of the sea at remote periods. It appears that there is a want of horizontality in these lines. Thus the superior line of the Alten Fiord, where it attains a height of 67 yards, gradually becomes lower towards the open sea, and has only a height of 28 yards in the neighbourhood of Hammerfest. These results are of a nature to interest geologists (L'Institut).

16. *Oxides of Tin and Copper in Spring Water.*—Berzelius, in an analysis of spring water from Saidschütz, in Bohemia, discovered traces of oxide of tin and of oxide of copper, which he thinks may be traced to the olivine of the plutonic rocks from which it flows; the olivine, he adds, affording traces of these two oxides.

17. *Analysis of Fossil Bones.*—Von Bibra has published

an analysis of fossil bones from Schrebheim, near Schweinfurt. In these bones, the bone earth was entirely removed, so that they exhibited the same relations as are observed in the upfilling of crystals, in the so-called after crystals. The bones were imbedded in Keuper limestone, and afforded the following ingredients:—sulphuric acid, 3.437; carbonic acid, 4.400; siliceous acid, 9.600; alumina, 63.400; lime, 3.589; magnesia, 0.294; ox. manganese, 5.954; fluor, 4.327; water, 5.00.

18. *On the Minute Animals found in Substances during the Process of Putrefaction.*—During the putrefaction of animal substances, their elements are in a state of incessant change, and in a condition of disturbed equilibrium, which is altered and modified by the feeblest of the forces operating on it by foreign materials, and foreign affinities and temperature. Such a condition appears to afford the most fruitful field for the development of imperfect, and of the lowest classes of animals, and of microscopic animals, whose ova, as is well known, are every where distributed in the most inconceivable manner; they are developed in myriads in these putrefying substances, and they propagate themselves in myriads, for they appropriate to their nourishment the products of putrefaction. Many naturalists regard the chemical process of putrefaction as a mere consequence of the production of these animals; this is just as natural as it would be to ascribe the putrefaction of wood and its decay, to the plants for whose nourishment the decomposing soil serves. But these animals are not produced in decomposing substances, when the condition of its presence, viz. contact with the atmospheric air, is cut off, just in the same way as mites do not appear in cheese when flies have no access to it. This opinion falls to pieces of itself when we consider, that, on the disappearance of the putrefying body, the animals die, that, after their death, there must be a cause present which produces the destruction of their organization, and which determines the conversion of the component parts of their muscles and organs into new solid and gaseous products. This cause is, therefore, ultimately a chemical process.—(*Professor Liebig in Poggendorff's Annalen.*)

NEW PUBLICATIONS.

1. *Journal of the Asiatic Society of Bengal.* Edited by theActing Secretaries.

The number for March contains the following scientific articles, 1. *On three species of Musk* (MOSCHUS) *inhabiting the Himaleyan districts*, by B. H. Hodgson, Esq. 2. *On Isinglass in Polnemus sele, Buchanan, a species common in the Estuaries of the Ganges*, by J. M'Clelland, Assist.-Surgeon (*with a plate*). 3. *Captain Pemberton's Mission to Bootan in* 1837-38. 4. *Report on the Museum of the Asiatic Society*, by William Jameson, Esq., Assist.-Surgeon. 5. *Meteorological Register kept at the Assay Office for month of March* 1839. Number for April contains, 1. *Continuation of Captain Pemberton's Mission to Bootan.* 2. *Remarks upon the Rain and Drought of the last eight seasons in India.* By the Rev. R. Everest, Landour. 3. *Dr D. Stewart's Record of the Duration of* 13,019 *fatal cases in Hindoos.* 4. *Descriptions of four new species of Otter.* By B. H. Hodgson, Esq., resident at Catmandu, Nepal. 5. *On the Geographical Distribution of the Vulturidæ, Falconidæ, and Strigidæ, being the first of a series of Memoirs intended to illustrate the Geographic Distribution of the Ornithological Kingdom.* By William Jameson, Esq., Assist. Surgeon Bengal Medical Service, &c. 6. *Meteorological Register.* Number for May contains, 1. Lieut. Kittoe's *Journey through the Forests of Orissa.* 2. *Papers relative to the new Coal-field of Tenasserim.* 3. *On the State of Medicine in Egypt.* 4. *Note on the Dissection of the Arctonix collaris, or Sand Hog.* By G. Evans, Esq., late Curator of the Asiatic Society. 5. *On the Cultivation of Roses, and the Manufacture of Rose-water and Utur at Ghazeepore.* 6. *On the Museum of the Asiatic Society.* By Dr M'Clelland. 8. *Observations on Mr Jameson's Report on the Museum of the Asiatic Society.* By J. T. Pearson, A. S. B., and formerly Curator of the Museum of the Asiatic Society. 9. *Proceedings of the Asiatic Society.* 10 *Meteorological Register.*

2. *Madras Journal of Literature and Science.* Number 2d, year 1839, from April to July.

The following are the papers contained in this number:—

1. *Notes on the Duty of Government in Periods of Famine.* By John T. Thomas, Esq., Madras Civil Service. 2. *Observations on the Direction and Intensity of the Terrestrial Magnetic Force in Southern India.* By Thomas Glanville Taylor, and John Caldecott, Esqrs. 3. *Investigation of the nature and optical efficiency of the combination of Mirrors used to augment the illuminating powers of the Madras Light.* By Captain J. F. Smith, Engineers, F.R.S. 4. *On the Crystalline Structure of Trap Dykes, in the Syenite of Amboor.* By R. B. Smith, Lieutenant, Madras Engineers. 5. *Notice of River Dunes, on the banks of the Hogri and Pennaur.* By Lieutenant Newbold, A.D.C. to Major-General Wilson, C.B. 6. *On the Table-land of Cumbaucum Droog.* Communicated by the Madras Government. 7. *Remarks on Colonel Reid's Law of Storms.* By T. G. Taylor, Esq. Honourable Company's Astronomer. 8. *Report on the Statistics of the Four Collectorates of Dukhun, under the British Government.* By Lieutenant-Colonel Sykes. 9. *Horary Meteorological Observations, made agreeably to the Suggestions of Sir J. Herschel.* 10. *Meteorological Register.*

3. *Asiatic Researches*, Part II. vol. xx. Calcutta, 1839.

This part contains the following Memoirs:—1. *On the Government of Siam.* By Captain James Low, M. A. S. C. 2. *Notices of the Life of Skákya, extracted from the Tibetan authorities.* By M. Alexander Csoma Körösi, Siculo-Hungarian of Transylvania. 3. *Review of the Naishadha Charita, or Adventures of Nala Raja of Nishadha; a Sanscrit Poem.* By the Rev. W. Yates. 4. *On Siamese Literature.* By Captain James Low, M. A. S. C. 5. *Analysis of the Second Division of the Tibetan Work, entitled the Kah-Gyur.* By M. A. C. Körösi, Siculo-Hungarian of Transylvanian. 6. *Abstract of the contents of the Bstan-Kgyur.* By M. A. C. Körös.

4. *The Zoology of the Voyage of H. M. S. Beagle,* No. I. Part IV. *Fish.* By the Rev. Leonard Jenyns, A.M., F.L.S. London: Smith, Elder and Co.

This number is illustrated by eight uncoloured plates of the Acanthopterygii, and forms an interesting addition to this department of the natural history of fishes.

5. *Illustrations of the Zoology of Southern Africa.* By Dr Smith. London: Smith, Elder and Co.

Number VI. contains coloured representations of the following species of Macroscelides, viz. typicus, rupestris, intufi, brachyrynchus, and Edwardii: of *Birds*, the following species of Pyrrhulauda, viz. australis, verticalis, and leucotis: of *Reptiles*, the Testudo semiserrata. Number VII. contains,—of *Mammals* the following, viz. Cynictis Ogilbyii, and lepturus: of *Birds*, Platyrhynchus capensis; Saxicola infuscata; Alauda ferruginea; Ploceus subaureus; Pteroeles gutturalis; Parra capensis: of *Reptiles*, Vipera caudalis: of *Fishes*, Agriopus spinifer. Number VIII.—*Mammals*, Rhinoceros simus: *Birds*, Athene capensis; Dierurus Ludwigii; Corythaix porphyreolopha; Turdus obscurus, strepitans, libonyana, and guttatus: *Reptiles*, Testudo verroxii: *Fishes*, Carcharodon capensis. Number IX.—*Mammals*, Petromys typicus: *Birds*, Aquila coronata, bellicosa, Swainsonii; Strix capensis: *Reptiles*, Python natalensis: *Fishes*, Tilapia Sparrmanii. Number X.—*Mammals*, Euryotis irroratus, unisuleatus, Brantsii: *Birds*, Melaconotus similis; Lamprotornis Burchellii; Cossypha humeralis; Erythropygia pectoralis, and paena: *Fishes*, Leptorhynchus capensis.

6. *Illustrations of the Breeds of the Domestic Animals of the British Islands.* By David Low, Esq., F.R.S.E., Professor of Agriculture in the University of Edinburgh. Part I. Folio. Longman, Orme, Brown, Green, and Longman.

Most of our Edinburgh readers are doubtless aware of the existence of an interesting and important Agricultural Museum in the University. The collection has been formed under the able superintendence of Professor Low, and although not yet completed, is already extensive. It includes an admirably executed series of paintings of the various races of our domestic animals, and it is from them that the present lithographic illustrations have been derived. We shall notice the work more in detail when it has made some progress; and would merely state at present, that it is got up in a very beautiful and handsome style; and that the letter-press is not only very valuable to the agriculturist, but also of a nature to interest the general reader.

7. *Traité Eléméntaire de Conchyliologie, avec l'application de cette Science à la Géognosie.* Par G. P. Deshayes. Livraisons, 1—5. 8vo. Crochard.

In a former number (vol. xxvi. p. 212), we noticed with great commendation the first part of this work, and we at the same time gave a sketch of the plan which the author intended to follow. The four succeeding parts fully justify the high expectations we had formed from the specimen then before us, and from the distinguished reputation of M. Deshayes. The 1st number contains a chapter of general considerations on Conchology and the commencement of the 2d chapter, which embraces an admirable digest of the history of Recent and Fossil Conchology, and is concluded in the 4th number. The 5th and 6th numbers contain portions of the third chapter of the introductory matter, being a masterly essay on the comparative anatomy of the Mollusca; and likewise the beginning of the systematic treatise on the families and genera. The information communicated in the descriptions and general observations is clear, full, and quite up to the present state of science. The following genera have already been treated of: Aspergillum, Clavagella, Gastrochæna, Septaria, Teredo, and Teredina. Forty plates have appeared in the five numbers, and their careful execution and beautiful colouring are worthy of the Text.

List of Patents granted for Scotland from 21*st December* 1839 *to* 18*th March* 1840.

1. To James Hay of Belton, in the county of Haddington, Scotland, captain in the Royal Navy, an improved plough—"The Belton Plough"—23d December 1839.

2. To Christopher Nickels, of the York Road, Lambeth, in the county of Surrey, gentleman, being a communication from a foreigner residing abroad, "improvements in propelling carriages."—24th December 1839.

3. To Joseph Gibbs of Kennington, in the county of Surrey, engineer, "an improvement or improvements in the machinery for preparing fibrous substances for spinning, and in the mode of spinning certain fibrous substances."—24th December 1839.

4. To Thomas Edmondson of Manchester, in the county of Lancaster, clerk, "certain improvements in printing presses."—31st December 1839.

5. To James Nasmyth of Patricroft, near Manchester, in the county of Lancaster, engineer, "certain improvements applicable to railway carriages."—31st December 1839.

6. To THOMAS LAURENTE LAMY GODARD of Christopher's Street, Finsbury Square, in the city of London, merchant, being a communication from a certain foreigner residing abroad, "improvements in looms for weaving to be worked by steam or other power."—8th January 1840.

7. To JOHN BRADFORD FURNIVAL of Street Ashton, in the county of Warwick, farmer, being a communication from a certain foreigner residing abroad, "improvements in apparatus or materials to prevent persons and quadrupeds sinking when in the water."—8th January 1840.

8. To WILTON GEORGE TURNER, late of Park Village, Regent's Park, in the county of Middlesex, but now of the town and county of the town of Newcastle-upon-Tyne, Doctor of Philosophy, and HERBERT MINTON of Longfield Cottage, in the parish of Stoke-upon-Trent, in the county of Stafford, manufacturer, "an improved porcelain."—9th January 1840.

9. To RICHARD BEARD of Egremont Place, New Road, in the county of Middlesex, gentleman, being a communication from a certain foreigner residing abroad, "improvements in printing calicoes and other fabrics."—9th January 1840.

10. To ALEXANDER FRANCIS CAMPBELL of Great Plumpstead, in the county of Norfolk, Esq., and CHARLES WHITE of the city of Norwich, mechanic, improvements in ploughs, part of which improvements are applicable to harrows and other agricultural implements."—9th January 1840.

11. To ROBERT MONTGOMERY of Johnstone, in the county of Renfrew, in the kingdom of Scotland, gentleman, an improvement or improvements in spinning machinery applicable to mules, jennies, slubbers, and other similar mechanism."—9th January 1840.

12. To WILLIAM VICKERS of Firs Hill, in the county of York, steel manufacturer, being a communication from a foreigner residing abroad, "an improvement in the manufacture of cast-steel."—10th January 1840.

13. To CHRISTOPHER EDWARD DAMPIER, of Ware, in the county of Hertford, attorney-at-law, "an improved weighing-machine."—14th January 1840.

14. To JOHN LESLIE of Conduit Street, Hanover Square, in the county of Middlesex, tailor, being a communication from a certain foreigner residing abroad, "improvements in measuring the human figure."—15th January 1840.

15. To WILLIAM HARPER of Couper's Court, Cornhill, in the city of London, patent stove manufacturer, and THOMAS WALKER of Birmingham, in the county of Warwick, "improvements in stoves and grates, and in preparing metal plates for such stoves and for other purposes."—15th January 1840.

16. To MATTHEW HEATH of Furnival's Inn, in the city of London, gentleman, being a communication from a certain foreigner residing abroad, "improvements in clarifying and filtering water, beer, wine, and other liquids."—15th January 1840

17. To THOMAS CLARK and CHARLES CLARK of Wolverhampton, in the county of Stafford, ironfounders and copartners, "improvements in glazing and enamelling cast-iron hollow ware."—15th January 1840.

18. To JOHN AINSLIE, farmer, Redheugh, near Dalkeith, "a machine for

a new and improved mode of making or moulding tiles, bricks, retorts, and such like work, from clay."—20th January 1840.

19. To SAMUEL WHITE WHITE of Charlton Marshall, in the county of Dorset, Esq., "improvements in preventing persons from being drowned."—20th January 1840.

20. To ARTHUR ELDRED WALKER of Melton Street, Euston Square, engraver, "improvements in engraving by machinery."—20th January 1840.

21. To ROBERT LORIMER, brassfounder, No. 45 Jamaica Street, Glasgow, in the county of Lanark, North Britain, "an improvement or improvements in stoves."—25th January 1840.

22. To MILES BERRY of the office for Patents, 66 Chancery Lane, in the county of Middlesex, patent agent, being a communication from a foreigner residing abroad, "an invention or discovery by which certain textile or fibrous plants are rendered applicable to making paper, and spinning into yarns, and weaving into cloth, in place of flax, hemp, cotton, and other fibrous materials, commonly used for such purposes."—27th January 1840.

23. To JOHN JONES of Westfield Place, Sheffield, in the county of York, "a new frying and grilling pan, for the cooking of steaks, chops, and other meats."—29th January 1840.

24. To ROBERT HERVEY of Manchester, in the county of Lancaster, dry-salter, "certain improvements in the mode of preparing and purifying alum, alumina, aluminous mordants, and other aluminous combinations and solutions, and the application of such improvements to the purposes of manufacture."—31st January 1840.

25. To FRANCIS WORRELL STEVENS of Chigwell, in the county of Essex, schoolmaster, "certain improvements in apparatus for propelling boats and other vessels on water."—1st February 1840.

26. To WILLIAM ISAAC COOKSON of Newcastle-upon-Tyne, Esquire, "certain improved processes or operations for obtaining copper and other metals from metallic ores."—3d February 1840.

27. To GEORGE WILSON of St Martin's Court, St Martin's Lane, in the county of Middlesex, stationer, "an improved paper-cutting machine."—3d February 1840.

28. To MILES BERRY, of the office for Patents, 66 Chancery Lane, in the county of Middlesex, patent agent, being a communication from a foreigner residing abroad, "certain improvements in machinery, or apparatus for making or manufacturing pins, and sticking them in paper."—5th February 1840.

29. To GODFREY ANTHONY ERMEN of Manchester, in the county of Lancaster, cotton spinner, "certain improvements in machinery or apparatus for spinning, doubling or twisting cotton, flax, wool, or other fibrous materials, part of which improvements are applicable to machinery in general."—5th February 1840.

30. To JAMES CAPPLE MILLER of Manchester, in the county of Lancaster, gentleman, "certain improvements in printing calicoes, muslins, and other fabrics."—7th February 1840.

31. To JOHN ALEXANDER PHILIP DE VAL MARINO of No. 17 Clifford Street, Bond Street, in the county of Middlesex, Esquire, "certain improve-

ments in the manufacture of gas, and in the apparatus employed for consuming gas for the purposes of producing light."—7th February 1840.

32. To JOHN FRANCOIS VICTOR FABIEN of King Street, in the city of London, gentleman, being a communication from a certain foreigner residing abroad, "improvements in pumps."—12th February 1840.

33. To JONATHAN FELL of Workington, in the county of Cumberland, ship-builder, "improvements in building ships and other vessels."—12th February 1840.

34. To JOHN REYNOLDS of Victoria Hotel, Euston Square, in the county of Middlesex, Esquire, "improvements in the manufacture of salt."—12th February 1840.

35. To HENRY PINKUS, late of Pensylvania, in the United States of America, but now of 77 Oxford Street, in the county of Middlesex, gentleman, "improvements in inland transit, some of which improvements are applicable to, and may be combined with, an improved method of, or apparatus for, communicating and transmitting or extending motive power, by means whereof carriages or waggons may be propelled on railways or roads, and vessels may be propelled on canals."—19th February 1840.

36. To JAMES BEAUMONT NEILSON of Glasgow, gentleman, "certain improved methods of coating iron under various circumstances, to prevent oxidation or corrosion, and for other purposes."—25th February 1840.

37. To WILLIAM PONTIFEX of Shoe Lane, in the city of London, "an improvement in treating fluids containing colouring matter, to obtain the colouring matter therefrom."—3d March 1840.

38. To THOMAS ORAM of 27 East Street, Red Lion Square, in the county of Middlesex, gentleman, "improvements in the manufacture of fuel."—5th March 1840.

39. To WILLIAM FORRESTER, residing in Barrhead, in the county of Renfrew, manager at Levern Mill, "certain improvements in sizing, starching, and otherwise preparing warps for weaving fabrics, and in the machinery or apparatus therewith connected."—6th March 1840.

40. To JOHN BURN SMITH of Salford, in the parish of Manchester, in the county of Lancaster, cotton spinner, "certain improvements in machinery for preparing, roving, spinning, and twisting cotton and other fibrous substances."—6th March 1840.

41. To LAURENCE WOOD FLETCHER of Chorlton-upon-Medlock, Manchester, in the county of Lancaster, machinist, "an improvement or improvements in the manufacture of woollen and other cloths or fabrics, and in the application of such cloths or fabrics to various useful purposes."—14th March 1840.

42. To JOSEPH SCHOFIELD of Littleborough, in the county of Lancaster, cotton-spinner and fustian manufacturer, and EDMUND LEACH of Littleborough aforesaid, manager of cotton spinners and weavers, "certain improvements in looms for weaving various kinds of cloth."—18th March 1840.

43. To WILLIAM MALTBY junior, of Mile End, chemist, and RICHARD CUERTON, brassfounder, of Percy Street, in the county of Middlesex, "improvements in extracting and concentrating the color, tanin, and other matters contained in vegetable and animal substances."—18th March 1840.

INDEX.

Aërolite, notice respecting the fall of one near Juvinas in France, 385.
Agassiz, his observations on the scales of fishes, in reply to Mandl's observations on that subject, 287.
Andalusite, same species with chiastolite, 417.
Antarctic expedition, instructions in zoology and animal physiology for, 72.
Arches, oblique, essay on, by Edward Sang, 301.
Asiatic Researches, noticed, 423.
Asiatic Society of Bengal, Journal of, 200, 422.

Baer, K. E. Von, his sketch of animal life in Nova Zembla, 93.
Barry, Dr, Royal Society gold medal presented to him, 196.
Batrachians, on the suspension of life in, from the effects of cold, 71.
Beaver, observations on the, 68.
Beck, Van, on the temperature of plants, 331.
Berzelius, Jacob, his remarks on Professor Keilhau's theory of granite, 371.
Bischof, Gustav, on the inflammable gases of the Prussian coal-mines, 183.
Bones, fossil, observations on, 420.
Boracic acid lagoons, account of, by Dr Bowring, 88.
Botryolite and Datolite, their chemical composition, 417.
Bowring, John, his account of the boracic acid lagoons of Tuscany, 85.
Brown, Robert, LL.D., presented with the Copley medal, 197.
Bruce, C. A., superintendant of tea-culture, on the manufacture of tea, &c. 126.
Buckland, Dr, his address on delivering the Wollaston medal to M. Dumont, and a year's proceeds of the Wollaston Fund to Mr Sowerby, 379.

Chiastolite and Andalusite, the same species, 417.
Chloritoide, a mineral species noticed, 419.
Christison, Dr, his observations on the circumstances modifying the physiological actions of plants, 405.
Cinnamon, observations on, by H. Marshall, F. R. S. E., Deputy Inspector-General of Hospitals, 27.
Coast-line of Fuci, height of, in Alten Fiord, 420.
Connell, A., Esq. F. R. S. E., his analysis of the new mineral named Greenockite, 392.
Cretaceous rocks, their microscopic organic remains, examination of, by M. Ehrenberg, 161.
Cunningham, R. H., on granite, 407.

Daguerreotype, observations on, by Dr Fyfe, 205.
Darwin, Charles, Esq., his "journal and remarks," 201.
Datolite and Botryolite, their chemical composition, 417.

Deshayes, his Traité Elementaire de Conchyliologie, noticed, 425.
Dutrochet, M., experiments on the temperature of vegetables with the thermo-electric apparatus, 103.

Ehrenberg, C. G., Professor, on the calcareous and siliceous microscopic animals which form the chief component parts of cretaceous rocks, 161.
Electrical currents in metalliferous veins, remarks on, by R. Were Fox, Esq., 267.
Electrical currents in metalliferous veins, researches on, by Professor Reich, 1.

Fishes, scales of, described, by L. Mandl, M. D., 113, 274—remarks on the scales of, by M. Agassiz, in reply to Mandl, 287.
Fluoric acid, on its supposed existence in certain animal matters, by G. O. Rees, M. D., 90.
Fossil organic remains found in the great Southern Highlands of Scotland, 198.
Fox, Were, Esq., remarks on electric currents in metalliferous veins, 267.
France, diseases of, considered in their relation to its meteorological history, by Dr Fuster, 238.
Fusibility of minerals, scale of, proposed, 419.
Fuster, Dr, his observations on the diseases of France in their relation to the seasons, 238.
Fyfe, Dr Andrew, on daguerreotype, 205.

Glaciers of the Alps, observations on, by M. J. André de Luc, 15.
Graham, Professor, his descriptions of rare and new plants, 397.
Granite, theory of its formation by Keilhau, notes on, 366; remarks on Keilhau's theory of granite by Berzelius, 371.
Greenockite, a new mineral species belonging to the order Blende, described, 390—chemical analysis of, 392.

Hoffman, Professor, of Berlin, on rivers which break through mountain-chains, 42.
Human races, on the extinction of, by Dr Pritchard, 166.

Iceland moss, &c., on the removal of the bitter taste and lichenous odour of, 269.
Inflammable gases of the Prussian coal-mines, described, by Professor Bischof, 183.
Infusoria, the digestive organs of, considered, by Dr Meyen, 271.
Insects, their geographical distribution traced, 170, 224.
Instructions, zoological, for the French Scientific Expedition to the North of Europe, 53—also for the British Antarctic Expedition, 72.

Jackson, Dr, on the cultivation of roses, 326.

Keilhau, Professor, explanatory notes on his paper relating to granite, 366.—Remarks on his memoir on granite, by Berzelius, 371.—His answer to Berzelius, in a note, 378.

Kupffer, M. T., his meteorological observations made at St Petersburg from 1822 to 1835, 77.

Lamp, polyphotal, described, 193.
Lantan, a new metal, noticed, 416.
Laurus cassia of Linnæus, observations on, by Robert Wight, M.D., 20.
Logan, F., Surgeon R. N., on the habits of testaceous mollusca belonging to the genera Achatina and Phasianella, 47.
Low, Professor, his Illustrations of the Breeds of the Domestic Animals of the British Islands, noticed, 424.
Luc, J. A. de, on the glaciers of the Alps, 15.—On the transverse valleys or openings in chains of mountains, which afford a passage to rivers, 32.

Madras Journal of Literature and Science for 1839, noticed, 201, 422.
Malthacite, new mineral species, noticed, 418.
Mandl, Dr L., his researches on the structure of the scales of fishes, 113, 274.
Marshall, Henry, F.R.S.E., his observations on cinnamon, 72.
Meteorological observations of Kupffer at Petersburg, 77.
———— Tables for Applegarth, in Dumfriesshire, 112, 324.—for Kinfauns, 319.—for Edinburgh, 322.—for Alford, 320.—for Abbey St Bathan's, 323.—for Carlisle, 326.
Microscopical Society, their proceedings reported, 414.
Mikrolin, a new species of mineral, noticed, 417.
Mollusca, testaceous, on the habits of, by F. Logan, Esq. 47.
Monazite, the chemical composition of, noticed, 417.
Morgan, T., his observations on the digestive organs of the infusoria, 271.

Narrative of experiments and suggestions, by the late Mr James Taylor, for the application of steam in navigation, by Sir John Robison, 45.
Nile, its present and former levels considered, by Sir T. G. Wilkinson, 211.
Nova Zembla, sketch of animal life in, by M. K. E. von Baer, 93.

Oils, fixed, on the adulteration of, by Dr Davidson, 250.
Oil, palm, on its decolorization, by Dr Davidson, 256.
Oils, fish, the removal of the fœtid odours of, by Dr Davidson. 258.
Oils, report on Dr Davidson's experiments thereon, by Committee of Society of Arts, 264.
Opaline Allophane, account of, 418.

Patents granted for Scotland, from 17th September to 14th December 1839, 202.—from 21st December 1839 to 18th March 1840, 425.
Perikline, a species of felspar, noticed, 419.
Plants, temperature of, 103, 331.
Plumage, on the changes of, according to the seasons, 70.
Pritchard, Dr, on the extinction of human races, 166.

Quartz veins, effects of, described by Rozet, 395.

Redfield, M., his observations on the law of storms, 361.
Rees, G. O., M.D., on the supposed existence of fluoric acid, as an ingredient in certain animal matters, 90.
Register of mean temperature, &c. at Applegarth, by Wm. Dunbar, D.D., 112.
Reich, Professor F., researches on the electrical currents in the metalliferous veins of 'Himmelfahrt, near Freyberg," 1.
Robison, Sir John, on the application of steam to navigation, 45.—on the best means of burning gas for supplying heat, 291.—on the best means of burning gas for the purpose of illumination, 293.
Roses, on the cultivation of, 326.
Royal Society of Edinburgh, proceedings of, from December 1839 to February 1840, reported, 400.
Russell, J. Scott, F.R.S.E., his description of a polyphotal lamp, and reflector of single curvature, employed in steam-vessels and canal-boats, 193.

Salt, recent deposits of, 420.
Sang, Edward, civil-engineer, his essay on the construction of oblique arches, 301.
Scandinavia, its geology noticed, 387.
Serpentine, crystals of, 418.
Shaw, John, on the growth of the salmon, from the exclusion of the ovum to the age of two years, 400.
Snarum in Norway, observations on the serpentine of, 418.
Spring water containing oxides of tin and copper, 420.

Tea, on the manufacture of, and on the extent and produce of the tea-plantations in Assam, by C. A. Bruce, Esq., 126.
Temperature of vegetables, experiments on, by M. Dutrochet, 103.
Traill, Dr., on the means employed to prevent forgeries of bank notes, bills of exchange, checks, &c., 404.

Wernerian Natural History Society, proceedings of, 199, 407.
Wilkinson, J. G., his observations on the present and former levels of the Nile, 211.

Zoological instructions of the Royal Academy of Sciences, Paris, for the scientific expedition repairing to the North of Europe, by M. J. Geoffroy Saint Hilaire, 53.
——— for the British Antarctic expedition, 72.
Zoology of the voyage of H. M. S. Beagle, No. 1. Part IV. Fishes, noticed, 423,
Zoology of South Africa, 424.

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